

count of the discomfort the hernial appliance produced during the heated term it was removed, and when next seen the hernia was as large as the child's head.

An operation for the radical cure was deferred in the expectation of recurrence of the disease superseding its necessity, until after the operation. Finally to give relief to the inconvenience caused by the large hernia it was decided that an operation was demanded by the circumstances and justifiable. On December 9, 1896, the operation was performed without incident worthy of note save that the anesthetics were borne without any of the alarming symptoms caused by their use in the first operation.

The case proceeded well until the expiration of the second week, when a solid tumor was discovered deep in the abdominal cavity. This presaged the trouble that soon made its appearance. The tumor increased in size rapidly, without any indication of suppuration which

characterized the exudate in the former operation. But little pain was experienced by the child through the subsequent progress of the case. As the tumor increased in size, emaciation progressed until his death, June 22, 1897.

The cut presented indicates the condition of the child at that time.

The child had survived the first operation nearly one and a half years, and the second over six months.

A post mortem examination was made the day of death. The stomach, liver, remaining kidney and lungs were normal. There was some infiltration of the pleural cavity and peritoneal also of the cellular tissue of the lower extremities. Three solid tumors were found, the weight of which were respectively as follows: 14, $3\frac{1}{4}$ and $2\frac{1}{4}$ lbs., aggregating $19\frac{1}{2}$ lbs. These tumors originated in the lumbar glands to which they were firmly adherent. The abdomen measured $43\frac{1}{4}$ inches in circumference. Age at death 2 years, 9 months.

HYPERTROPHY OF THE LINGUAL TONSIL.

WILLIAM MARTIN, M.D., BRISTOL, PA.

Until recently very little attention has been given this subject, and, while during the last few years some study has been made of the lingual tonsil, its diseases have not claimed the attention they merited.

This paucity of literature cannot be attributed to the scarcity of such cases in an ordinary or special practice, but must be either from the fact that the condition has not been recognized or that the profession is not sufficiently alive to the importance of the condition. Its occurrence has been found to be at least in ten per cent. of all cases coming under the care of specialists for naso-pharyngeal treatment. I fully believe that this number is not exaggerated, for since this hypertrophy has been noticed to have such a bearing upon the condition of a number of my patients, I have made the practice of investigating more closely, with the result of finding from ten to twelve per cent. so affected. To illustrate that too little attention is

given to this subject, I will mention that in one of the "Systems of Rhinology and Laryngology" of recent date, the only allusion to the hypertrophy of the lingual tonsil that I could find was as follows: "On the base of the tongue are seen a group of glands constituting the so-called lingual tonsil, and sometimes so much enlarged as to press on the epiglottis and give rise to various morbid symptoms." To dismiss a subject of this kind in this summary way shows that the importance of this pathological condition has not been properly recognized.

The first mention of the hypertrophied lingual tonsil appears to be credited to Stoerck in 1877. During the following years several isolated cases have been reported, but until recently a thorough study of the tonsil had not been made. Its location is on the base of the tongue, being situated either in the centre as one mass, or upon either side of the median line in two or more separate masses. When

this hypertrophied condition causes it to impinge upon the epiglottis, either by projecting toward it, or downward into the space between it and the tongue, the symptoms to be mentioned later are the result. The tonsil is a part of the ring of lymphoid tissue that surrounds the pharynx, of which the faucial and the third tonsil or that of Luschka are parts, and, being of a like structure, is very prone to become involved in pathological conditions affecting the others. The usefulness of the gland may be summed up as follows, viz: To prevent lodgment of food in the space formed by the base of the tongue and the folds of the epiglottis; to lubricate by its secretions, thus assisting in the act of deglutition, and to moisten the circumvallate papilla, thus aiding in the sense of taste. Being exposed as it is in its position to irritation from various causes, its involvement in pathological changes is likely to occur, yet this tendency appears to be less decided than that of the faucial tonsils. An acute inflammation of the latter may involve the lingual, and a continuous inflammation or irritation is likely to result in the hypertrophy of lingual tonsil. This morbid condition is the one most frequently affecting it, although such diseases as diphtheria or syphilis may locate themselves in the gland. The etiology of the condition of hypertrophy is similar to that of the other tonsils. Females appear to be affected in greater proportion, so far as reported. Age is a decided factor. There appears to be no reports of cases preceding the age of puberty. This would suggest the absence of relationship with adenoids.

The symptoms that are of greatest importance and which attract attention to the probable existence of such a condition are the local ones. Reflex and constitutional symptoms, while often present, are of minor significance. The one local symptom that notifies the patient that something is wrong, is the sensation of a foreign body in the throat, that will not disappear with deglutition. This sensation varies in intensity and discomfort as well as the direction in which it extends. When the mass occupies the centre of the tongue, a sense of constriction is present. If the main involvement is on either side

of the median line, pain to a greater or less degree is usually felt. In my cases, this last feature has not been very decided. When present, it is usually during the act of swallowing. Dr. Lewis* has recently found that the hypertrophied lingual tonsil is a factor in producing varied pressure sensations where that condition was accompanied by goitre in the same patient, and advocates the examination of all such cases where the symptoms of goitre are out of proportion to the thyroid enlargement. Hoarseness, or laryngeal fatigue, from the mildest to the most decided type, may be considered another local symptom. Hemorrhage has been noticed as an accompaniment, but usually only sufficient to stain the saliva. This latter is not necessarily always present, but when the hypertrophy is associated with an enlargement of the veins on the dorsum of the tongue, this bleeding is very sure to occur. Cough is the principal reflex symptom, due to the local irritation, and is frequently of a dry, harsh nature, and at times persistent under ordinary internal medication. Anemia is often associated with this condition of hypertrophy, and may be considered a constitutional symptom.

The treatment of hypertrophy of the lingual tonsil does not vary much from that of the other lymphoid bodies. If the enlargement is not excessive, the condition may be remedied by local applications of iodin in various strengths. This is best applied in solution with glycerin by aid of potass. iodid. Some advocate nitrate of silver. Either of these require considerable care in the application, as a dropping of the remedies into the larynx is likely to produce a violent spasm of the glottis. When the hypertrophy is very large, and projects enough to impinge upon the epiglottis, the cold snare or galvano-cautery will be required. In the use of the latter great care must be used to prevent injury to the epiglottis. The bleeding following this method is usually slight, and only likely to be excessive when the hypertrophy is associated with varicosity of the veins of the dorsum of the tongue. As a rule, even this hemorrhage is controllable and should not deter one from relieving the patient from this uncomfortable and annoying condition.

CURRENT LITERATURE CONDENSED.

Analysis of Urine in Nephritis.¹

Diagnosis of diseases of the kidneys is made by analysis of the urine, and I give in a concise and practical way the results of my experience.

For examination a specimen from the entire twenty-four hours' secretion only should be used. No opinion can be based upon a specimen passed at any time of the day or night; and the morning urine, which was considered for so long a time the best analysis, is especially objectionable. My practice has been to direct the patient to send the entire production of the twenty-four hours. I can then measure it accurately. It is of great importance to instruct the patient as to the mode of collecting. The urine should be received in perfectly clean bottles, tightly corked, and kept in a cool place. Urine in a state of beginning fermentation is unfit for analysis. To prevent this I order 3*i* of chloral in solution to be placed in the bottles, and the contents to be gently shaken after each addition. The chloral will prevent the development of all bacteria, and the urine will keep at all temperatures for an indefinite period. It is useless to analyze urine with an alkaline reaction and full of bacteria, for the latter very rapidly eat up the casts and other microscopical elements of importance.

The various forms of nephritis which we differentiate pathologically cannot be so separated by analysis of the urine, and I think it most practical to consider only acute and chronic congestion, and acute and chronic nephritis. To diagnose any given form of inflammation of the kidneys it is necessary to bear in mind the entire clinical picture presented, and to make repeated and careful analyses. But even with the greatest care mistakes are frequent, because the urine in different forms of chronic inflammation presents often the same characteristics.

The points to be considered in examining the urine in suspected renal disease

are: Quantity, reaction, color, specific gravity, phosphates and chlorids, albumin, urea, and a microscopical examination of the sediment.

Quantity.—The average physiological quantity of urine excreted in twenty-four hours is about fifty ounces. This varies, however, according to the amount of fluid taken and the amount discharged by the bowels and skin. The urine is markedly diminished in all forms of acute nephritis, and in acute and chronic congestion of the kidney. The diminution is less in chronic diffuse nephritis, where the average amount falls to from thirty to forty ounces. The urine is increased in that form of nephritis characterized by waxy degeneration, and in some of the slowly developing forms of chronic nephritis.

Reaction.—The twenty-four hours' secretion of the urine is acid in reaction in health, as well as in the majority of kidney inflammations. Urine containing pus is either faintly acid, alkaline, or neutral when voided, but rapidly becomes alkaline on standing.

Color.—The color varies much in the different forms of nephritis. In all acute cases it is usually smoky or brown even to chocolate color, due to the admixture of blood which assumes a brown color in the presence of an acid. Should the urine be alkaline, as it is after the ingestion of food, then the admixture of blood imparts a red color. In acute and chronic congestion of the kidney the urine is usually smoky, but it may be clear at times when the blood-pressure is less in the kidneys. In chronic nephritis the urine is usually of a light straw color, and very clear, and this is especially the case in amyloid degeneration of the kidney. When the urine becomes scanty, however, in the chronic forms of inflammation, it is of dark amber color, owing to the high degree of concentration.

Specific Gravity.—In all forms of acute inflammation of the kidney the sp. gr. of the urine is high, 1030 or over, due in

¹ J. C. BIERWIRTH, M.D., in *Brooklyn Med. Jour.*

part to the high degree of concentration and in part to the admixture of blood. The same may be said of acute and chronic congestion of the kidney. In chronic nephritis the sp. gr. is below the normal and does not increase when the amount of urine is diminished. In the so-called cirrhotic kidney we find the sp. gr. usually below 1015, generally 1010 to 1012. A persistent low sp. gr. should always excite suspicion of an existing chronic nephritis, even if its presence cannot be demonstrated by the finding of albumin and casts. There is one point of importance to be borne in mind, however, in regard to the sp. gr. in chronic nephritis, and that is, that when marked lesions of the circulatory apparatus are added, the sp. gr. will be high when the blood-pressure of the kidneys becomes high.

Phosphates and Chlorids may be considered together. They are of no special clinical significance, except in so much as they indicate disturbances of the digestive apparatus. In the acute varieties of nephritis they are always relatively increased, due to the concentration of the urine. In all chronic cases, however, they are always diminished. Occasionally we find a case of chronic nephritis in which the sp. gr. will increase for a while, which increase is due to an increase in the phosphates and chlorids from some digestive disturbance, and not due to an increase in urea. This is important to bear in mind, because some physicians make a rough estimate of the urea from the specific gravity.

Albumin.—For purposes of diagnosis the test for albumin is perhaps the most important to be considered. Its importance may be inferred from the large number of tests devised for its detection, and from the large amount of literature on the subject. The source of albumin in the urine is the blood-serum, which transudes from the capillaries into the tubules of the kidney. The conditions which permit this transudation are changes in the walls of the capillaries, either inflammatory or not, changes in the composition of the blood, and changes in the blood-pressure. It is important to bear these different causes in mind, so as to be able to determine the clinical significance of albumin in the urine, which may exist

without any kidney lesions being present. This is now a well-known fact. Not so very long ago albumin in the urine was always considered *prima facie* evidence of an existing nephritis, and the larger the amount the graver the prognosis. When we think of the causes which permit a transudation of serum into the tubules, we will readily see that under the head of changes in the composition of the blood, many diseases, such as anemia, chlorosis, and eclampsia, will account for the presence of albumin in urine. But aside from these and kindred diseases we find not infrequently cases where albumin is present in the urine and in which we cannot find any evidence of nephritis. Delafield recognizes four varieties of albuminuria without nephritis.

1. *Paroxysmal or Cyclic Albuminuria.*—This form occurs regularly in young males, who also suffer more or less from disturbances of the general health. The quantity of albumin is large, while casts are few or absent; there is a regular rise and fall in the quantity of albumin at regular intervals in the twenty-four hours. It begins to appear soon after rising in the morning, increases through the day, falls after going to bed, disappears at night, and reappears again in the morning. This regular cycle can be disturbed by changing the hours of rest, of meals, and of exercise. The rule is, that the appearance of the albumin is favored by exercise and by eating, while rest in bed causes it to disappear. There seems to be no way of accounting for this form of albuminuria, except by supposing that there are changes in the composition of the blood or in the walls of the renal capillaries. These patients suffer from anemia, lose flesh and strength, have headaches, neuralgic pains, bodily and mental languor, hysteria, and disturbances of the functions of the liver, stomach, and intestines. But there is a great difference in the patients as to how far these additional symptoms are developed. In some they are but trifling, in others they are well marked. To distinguish these patients from those who have a true nephritis is by no means easy; the diagnosis may remain doubtful for months, and even then it is difficult not to make mistakes.

2. *Dietetic Albuminuria.*—This occurs

both in children and in adults. It may follow the ingestion of only certain kinds of food—cheese, pastry, eggs; or of any kind of food; or of food not properly digested; or it may occur when exercise follows immediately upon the ingestion of food. The quantity of albumin is small and there are few or no casts. If this form of albuminuria is temporary, it is not a serious condition, but if the disposition to it persists the patients are to be regarded with suspicion.

3. Albuminuria after Exertion.—The exertion must be prolonged and severe, such as long marches, violent exertion in walking, running, or athletic exercises. The quantity of albumin may be considerable and numerous casts may be present. It seems probable that this form of albuminuria is due to a congestion of the kidneys caused by exertion. After the cessation of the exertion the albumin regularly disappears within a few hours or days. But a repetition of such temporary congestions of the kidney might lead to the development of a true nephritis.

4. Single Persistent Albuminuria.—These patients may for years have small quantities of albumin nearly every day, but not at all hours of the day. The albumin is not abundant, it often disappears after rest; there may also be a few other symptoms of kidney disease, even when they are under observation for years. But one always feels anxious concerning such persons. Sooner or later they are apt to develop chronic nephritis, or endocarditis, or endarteritis.

The tests devised for the detection of albumin are too numerous to mention here. Many of them are objectionable because they precipitate in addition to albumin also much mucin (more properly called nucleo-albumin), peptone, alkaloids, and the pine acids, all of which except mucin are redissolved upon the application of heat. The contact method, by which most of the delicate tests are used, is objectionable because when the zone of opacity at the point of contact is very faint the heat often disturbs it and makes the result doubtful. I have for some years discarded all tests by contact, including Heller's test. The latter has been in use for the longest period per-

haps, and is objectionable because it precipitates mucin, and because it darkens the pigment of the urine at the point of contact, which makes a faint opacity to be seen only with great difficulty, and always leaves a doubt as to its existence. When an appreciable quantity of albumin is present, heat and nitric acid are sufficient to detect it with accuracy, but unfortunately the majority of cases of chronic nephritis only contain a very small amount, a trace, and here the detection is not so easy. Mucin or nucleo-albumin is present in all urines, and is precipitated by most of the tests, and we are therefore often in doubt whether a resulting opacity is mucin or albumin.

My method of testing for albumin has been for some years the employment of two tests, the heat and nitric acid, and the ferro-cyanid of potassium test. The urine should first of all be perfectly clear, and if not so, should be filtered through a double wet filter. It should be acid in reaction. A portion of it is then boiled in a perfectly clean test-tube and strong nitric acid added, about fifteen to twenty drops. If an appreciable quantity of albumin be present a turbidity will at once result; but if only a trace be present it may remain perfectly clear for five or ten minutes and then a turbidity will gradually appear. I usually cork up the tube and set it aside for from six to twelve hours, at the end of which time I usually find a precipitate. This then may be one of four things: (1) acid urates; (2) uric acid; (3) nitrate of urea; (4) albumin. The first three are redissolved by heat, and I therefore boil the tube a second time, and if then a turbidity remains it is albumin without a doubt. This test avoids the mucin reaction, because mucin is soluble in strong acid solution, hence the addition of the nitric acid in excess. The second test which I employ is the ferro-cyanid of potassium test. It should be used with care and with attention to detail as laid down in Purdy's "Practical Urinalysis," second edition, page 74. This test is very reliable, avoids the mucin reaction, and is delicate enough to detect very small traces of albumin.

Large quantities of albumin are found in all cases of acute nephritis, in acute congestion, and in waxy degeneration of

the kidney, and may be found in chronic congestion if the blood-pressure is very high. In cases of chronic nephritis the amount of albumin is usually small, and in the so-called cirrhotic kidney it may be only a faint trace, and may even be absent for a time.

Urea.—The next and most important ingredient in the urine to be tested for is urea, not so much for purposes of diagnosis, as for prognosis and treatment. It is only in recent years that attention has been given to the estimation of the daily excretion of urea, and even to-day by far the larger number of physicians ignore the subject entirely. The urine of every patient suffering from chronic nephritis should be frequently examined for urea, and a daily test should be made when cerebral symptoms are present; for upon the amount of urea and not the amount of urine excreted depend our treatment and prognosis. The amount excreted by a healthy adult is about 500 grains in twenty-four hours. The apparatus which I employ for its estimation is the one devised by Dr. Chas. A. Doremus, and the one by Dr. Edward R. Squibb. The former employs a solution of potassium hypobromite and the latter a solution of sodium hypochlorite. I prefer Doremus' apparatus because it is simpler and more quickly used; Squibb's I always employ when I wish to make two tests, in cases where the daily amount of urea is very low. The daily amount excreted is diminished in all cases of renal disease, whether acute or chronic. It is difficult to say how low the quantity may fall without producing cerebral symptoms. The tolerance of the urea in the blood varies much in different people; some give uremic symptoms when they excrete 200 grains, and I have seen a case in which the amount was only 35 grains in twenty-four hours, and no symptoms were manifest.

Sediment.—The last ingredient in the urine to be considered is the sediment, which is without doubt the most important for diagnosis. It contains all the pathologic elements which prove the existence of renal disease; chief of these are pus, blood-cells, renal epithelia, and casts. With the aid of the modern invention of the centrifuge the sediment may now be examined soon after voided,

which is of importance, because we then get it fresh and unaltered. If no centrifuge is at hand the urine should be allowed to settle in a conical glass. The urine to be examined for casts, etc., should always be acid and free from bacteria, as already stated above, for the latter rapidly destroy the morphological elements for which we are looking. Casts are dissolved in alkaline urine.

In acute nephritis we find renal epithelia, red and white corpuscles, hyaline, granular, epithelial, and blood-casts. In the chronic cases the hyaline casts predominate and only an occasional granular or epithelial cast is found. In cases of contracted kidney we often find only a very few hyaline casts, of so delicate a structure that they are not visible by a strong light, but by darkening the field a little they are readily made out. In amyloid degeneration of the kidney we have waxy casts predominating, with a few hyaline and granular casts added.

Present Status of Inoculation Against Yellow Fever.²

The attention of the medical profession has been called anew to the preventive treatment of yellow fever by the claim of Giuseppe Sanarelli to have discovered its germ.

We find in the *Medical News* (January 27) the following notice: "A cable dispatch from Rio de Janeiro to the *London Times* states that a young Italian, Giuseppe Sanarelli by name, has undoubtedly discovered the yellow fever germ. Sanarelli is the director of the Uruguayan National Institute of Experimental Hygiene, and a follower of Pasteur. He is said to have sent an account of his discovery under seal to the Academy of Medicine at Rome."

There appears also in the *Medical Record* (April 24, 1897), the following allusion to this supposed discovery: "The Rome correspondent of the *Lancet* writes that Dr. Giuseppe Sanarelli, who believes that he has discovered the bacillus of yellow fever, as well as a remedy for the disease, has embodied his researches in a monograph which has for some time been in the possession of the Academia Medica

² J. MCFADDEN GASTON, M.D., in *Journal Amer. Med. Assoc.*

di Roma, that body being thus empowered to protect his claim to priority should that claim in the interval have been challenged. The supplementary studies by which he has sought to check the laboratory and clinical work leading up to his discovery are now completed, and the whole series, including the substance of the monograph aforesaid, will be published in three successive numbers of the *Annali dell' Instituto Pasteur*. Coincidently with the appearance of the first of these fasciculi, that is, within a few weeks' time, he will deliver before the Montevidean Society of Medicine a lecture, accompanied by illustrative preparations, in which the nature of his discovery and the successive stages by which it was arrived at will be fully set forth."

In connection with these announcements, I would ask attention to the following paragraph in the *Journal of the American Medical Association* headed "Prophylaxis of Yellow Fever in Brazil": "The disease was kept under control and stamped out in Buenos Ayres by the immediate removal of every case, as it appeared, to the floating hospital, while all the other inmates of the house were taken at the same time to the island quarantine station, where they remained until all danger was past. The house was submitted to a vigorous disinfection during the entire time of their absence, and as a final measure, all the walls were calcined. The physician first called to the case was also quarantined in his own house for the same length of time, and the premises disinfected. By these measures the disease was restricted to a few isolated cases in the early spring. Dr. A. Simoes proclaims in the *Rev. med. chir.* of Brazil the success of inoculations with attenuated cultures of the yellow fever microbe, the *micrococcus xanthogenicus* discovered by Freire in 1880. Since he was authorized in 1883 by the government to administer them, 11,881 persons have been inoculated, and he states that the mortality among them has been only 0.5 per cent., while among the rest of the population it has averaged 30 per cent. The yellow fever hospital had a mortality of 78 per cent. until the inoculations were introduced, when it fell to 30 per cent. He adds that if all the strangers

recently arrived and all persons exposed to contagion would have themselves inoculated epidemics would cease and there would be only a few sporadic cases, while vigorous hygienic and disinfecting measures in addition would exterminate the pest altogether.—*Cronica Medica*, October 15."

To enable those who may not have noted the progressive steps in yellow fever inoculation it is proper to state that in 1885 I drew the attention of Dr. Joseph Holt of New Orleans to the great practical importance of this matter; and as president and representative of the Louisiana State Board of Health he presented a memorial from the New Orleans Cotton Exchange to the American Public Health Association, to appoint a commission to investigate the discovery by Domingos Freire of a method for the prevention of yellow fever.

In connection with the discussion of this proposition in the *New Orleans Medical and Surgical Journal* the editor said: "It is our conviction that a few months will see Freire and his researches consigned to that lunar limbo where are laid away all things lost or forgotten here on earth."

But it appears that Freire, like Banquo's ghost, will not down, and after seventeen years the work goes on successfully in Brazil, and the Argentine Republic may well profit by the latter's example instead of trusting to quarantine and disinfection alone. It is to be hoped that Sanarelli will be as successful in Uruguay as Freire and Simoes have been in Brazil.

I subsequently presented a memorial to Congress from the American Medical Association and it was favorably considered, but upon getting the sanction of the Association at its next meeting for sending two additional commissioners to assist Dr. Sternberg with his work in Brazil and Mexico, Dr. J. B. Hamilton managed to have the resolution rescinded. The same gentleman, as chairman of the Section on State Medicine at the 1890 meeting in Nashville initiated steps to put a quietus to further investigation of the prophylactic inoculation of Freire against yellow fever, and with him and Dr. Sternberg rests the responsibility of the failure to secure its advantages in this country. It

is hoped that the influence of these prominent members of the profession may yet be counted in the interest of yellow fever inoculation. Great would have been the necessity for all precautions in view of the recent outbreak of yellow fever in Cuba and of the close relations of this island with Key West and Tampa, Fla., during the present civil war for Cuba's rights, not to speak of past epidemics at Brunswick, Jacksonville and New Orleans. At the meeting of the quarantine conference in Montgomery, Ala., in 1889, I claimed that as germane to the objects of the quarantine conference yellow fever inoculation should be accorded the place of the most radical means of prevention known, and that if it accomplished anything it accomplished everything.

In an article which appeared in the *Journal of the American Medical Association* (March 22, 1890), it was urged as a fact versus fiction. Again, as late as Dec. 8, 1894, I wrote for the *Journal of the American Medical Association* the conclusions which had been formed in September, 1894, in the International Congress of Hygiene and Demography, assembled at Buda-Pesth, Hungary. This Congress announced its belief in the discovery of Freire as follows: "It is a fact attained for science, thanks to the labors of Prof. Domingos Freire, supported by many physicians of Brazil and by observers of other countries, that a microbe (*cryptococcus xanthogenicus* *Freire ou bacteria de le Dantec*) is the active cause of yellow fever."

From Dr. Sternberg's instructive and interesting description of the observations made by himself and others, it is inferred that the germs or bacteria presented by Freire, Carmona, Gibier, Findlay and others are not to be regarded as identified uniformly in connection with yellow fever, so that the yellow fever bacillus has yet to be discovered. It is most probable that the views of Dr. Vaughan in regard to certain diseases being developed by ptomaines after the death of bacteria in the different structures, may be the key to explain the phenomena of yellow fever. All are aware that in the case of hydatids, the chief element of disorder is their death, and hence it would seem that the most philosophic investigation,

after the progressive changes in yellow fever have resulted fatally, should not detect living bacteria in the structures of the body, but that they should be sought in the fluids or secretions of the subject of yellow fever in its early stages.

Facts are stubborn things, and as in medicine we use many remedies empirically, without having any rational explanation of their *modus operandi*, we are called upon to test the prophylactic virtues of yellow fever inoculation without having a truly scientific clue to its efficacy. If it appears that inoculation in any form or with any substance, be it a bacillus or not, prevents or modifies the progress of yellow fever in human beings, we are warranted in resorting to it to secure our people against the ravages of this disease. It is claimed that such a process has been employed with satisfactory results in Rio de Janeiro by Dr. Domingos Freire, and the results have been tabulated, giving the names and location of those inoculated, reaching above seven thousand individuals, subject to the scrutiny of interested observers, before 1889. The deaths among all those subjected to this process have not exceeded one to the thousand, while the percentage of deaths among those attacked with yellow fever in the same localities has far exceeded this proportion, being one in one hundred. Under such circumstances we must either disprove the statistics or accept the results as conclusive. Dr. H. M. Lane, a former resident of Carthage, Mo., and now at the head of one of the largest educational institutions in Brazil, situated at San Paulo, was inoculated in 1886. He gave his report (*Journal of the American Medical Association*, Vol. vii, p. 165) in a paper on "Yellow Fever in Brazil." He was then a resident of Rio de Janeiro, Brazil, and had every opportunity for judging of the merits of inoculation. He had no great disturbance of the vital functions from inoculation, and has remained immune. The statistics of vaccination have never been questioned because the disease has never been explained by the culture of a microbe associated constantly with the disease, and recent studies presented to the consideration of the American Association of Physicians have failed to isolate the cause of variola.

The views of an editorial which appeared in the *Journal of the American Medical Association* some years ago, made this very point with reference to yellow fever inoculation and the studies that have been shown by Freire and Findlay. The mosquito was shown by the latter to have inoculated the virus of yellow fever, and the more frequent number of inoculations caused a gradual attenuation of the virus. The people thus bitten were affected with yellow fever in a mild form. The idea of Freire is a similar one, but he cultivates the virus in a laboratory in such a way that he secures the prophylactic influence without the serious consequences of yellow fever.

When Jenner introduced vaccination, more than a century ago, the presumption against his process was stronger than that which exists at the present day against yellow fever inoculation, and it was necessary to determine by actual demonstration that the modified impression produced by vaccination really gave immunity from any serious effects of variola.

This running comment may serve somewhat as an introduction to the forcible words of the editor of the *Journal of the American Medical Association* (Jan. 11, 1890), and already epitomized. "And apropos of inoculations, the press of the country has again referred to the inoculations practiced at Rio de Janeiro, stating that the city council of that city has been so favorably impressed by their ascertained value as protecting against yellow fever, that \$600 a month has been voted for vaccinal establishments. To us, at a distance, it seems that the reasoning of Dr. Domingos Freire of Rio de Janeiro, Brazil, Dr. L. Girerd, late of Panama, and Drs. Carlos Findlay and Delgado of Havana, is sound and based on the reasoning applied to vaccination. From a case of specific yellow fever, blood is taken from the finger and a culture is made. The attenuated culture is used for inoculating. Natural result, a mild yellow fever, or planting corn that they may get corn, to use a homely simile. Dr. L. Girerd, while in that hotbed of yellow fever, Panama, inoculated himself and produced a mild yellow fever. In December, 1886, the *Canada Medical Re-*

cord published a translation of his paper. In Havana, Cuba, Dr. Carlos Findlay repeatedly has inoculated new arrivals, with the happiest results, *i. e.*, subsequent immunity in that hotbed of yellow fever. Again, 'like produces like.' That the blood of a patient suffering from specific yellow fever must be full of its germs goes without saying. In attenuating it, the cultures made *à la Pasteur*, the gentlemen named have worked on accepted lines. They are well-known writers and investigators, as native and foreign medical literature testifies. They have fixed on certain germs or microbes which, from their constancy in their cultures, they believe to be the specific germs. Many years ago, during a limited epidemic of yellow fever at Southampton, England, Dr. Hassell of that city detected an unknown germ or entity in the blood of his patients. Ziemssen refers to it. But be that as it may, from the blood of yellow fever patients the poison of the disease is obtained.

"To repeat, time, patience, and investigation will clear up the minor details, the great ones seem to be indisputable. That yellow fever is due to an entity or germ all students of yellow fever accept."

NOTE.—"At the request of Dr. Freire the government of Brazil has appointed a committee of seven prominent physicians to investigate his claims in regard to the micrococcus xanthogenous" (*Journal of the American Medical Association*, July 10, 1897, p. 92).

The Baby Detective.

This is the name given to a young woman who is employed by one of the hospitals of New York City to "run down" the parents of sick babies which are left in that institution. The "baby-detective" became necessary, because many parents deserted their children after placing them in the hospital.—*Med. News.*

The number of suicides in 1890 was 2404; in 1891, 3531; in 1892, 3860; in 1893, 4436; in 1894, 4912; in 1895, 5759, and in 1896, 6420, and these all occurred in the United States. What relation has this to the general business depression, and the consequent inability to provide for the family, if any, and is the same increase shown during other times of financial depression?—*Annals of Hygiene.*

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PHILADELPHIA, SATURDAY, NOVEMBER 13, 1897.

EDITORIAL.

THE METRIC SYSTEM: A TRADE DEMAND.

For at least fifteen years, the profession has given its official sanction to the use of decimal measures and weights, yet the old "apothecary" system remains in more general use, and there are not a few pharmacies at which it would be unwise to present a prescription written in decimal terms. At first sight, this fact would seem overwhelming testimony to the superiority of the older system, but a more thorough consideration of the subject does not warrant such a conclusion. That the representatives of a conservative profession have been able to install and continue the metric system in the official organ of *materia medica* indicates that the newer system has substantial points of advantage. Moreover, investigation of the attitude which the profession has assumed toward the metric system, reveals that the obstacles to be overcome is rather the tremendous inertia of habit than any positive resistance. Occasionally a man

is found who maintains the actual superiority of the apothecary's system, with its arbitrary units and irregular but easily divisible scale of progression. For the most part, however, we are told that the preference is on account of years of familiarity, or that the local druggist is not considered a safe dispenser of metric prescriptions; or, in the case of younger men, that they preferred the decimal system but had to learn the other because their professors were unwilling to make the effort necessary to change; or some other like reason is given with the frank admission of the superiority of the scale of ten, and of correlated units.

We call attention to the fact that scarcely any of the convenient, ready-made preparations of drugs are furnished in exact metric doses, and that a considerable market will be opened to the manufacturer who will first recognize this lack and will conform to, or even anticipi-

pate the demand for pills, tablet triturates and hypodermic tablets of exact and convenient metric dosage. A supply of such tablets is not absolutely lacking, but the very practical objection is met with, that coincident with the adoption of the metric system, has been observed a tendency to minuteness of dose. A personal letter to one of the prominent wholesale manufacturers of drugs, in regard to the advisability of adding a list of metric preparations, elicited the reply that such a move would be impracticable on account of the large number of extra formulæ which would be required. Yet we note that the same house lists several sizes of most of the common tablets, that several drugs are represented by ten or eleven tablets and that, in one case, sixteen tablets of the same drug are standard. Under the circumstances, it would seem possible to reduce the number of tablets representing fractions of a grain and to add one or two representing convenient doses in centigrams or milligrams, as the case might be.

We are inclined to believe that the manufacturers underestimate the demand which would be found for convenient metric preparations. So many physicians are content to buy five-grain tablets and think of them as thirds of a gram, or to overlook the slight inaccuracy in calling one quarter of a grain fifteen milligrams, that the wholesalers fail to realize the avidity with which accurate metric tablets and pills would be taken, if obtainable. We are personally informed of some twenty physicians—representing a very considerable minority of the total regular list of one city—who could each probably be counted on to use twenty-five dollars' worth of tablets alone, each year, if metric "sizes" were on the market. We are inclined to think this a very conservative estimate; it certainly is, unless these twenty physicians are much fonder of liquid prescriptions or extem-

porized pills rolling in a box of powder than we are.

But no such estimate adequately represents the actual demand for metric dosage. It takes into account only those personally known as using the metric system in spite of the practical difficulties still existing. It omits entirely the very considerable number of physicians who are not willing to lead in such an undertaking but who only wait the removal of obstacles from their path to follow gladly.

It ought not to be necessary to point out that one of the essentials of business success is to anticipate a demand, to create a market, to make smooth the way for the consumer, and, more than anything else, to be the first to recognize a developing opportunity.

As to the merits of the metric system, it is late in the day to speak. The arguments on this question have been threshed over and over again, and whatever one's personal opinion may be, we need now only point to the fact that the metric system is not merely permitted but authorized as the sole official basis of preparing drugs, although all possible pains have been taken by the committee for revision of the *Pharmacopeia*, to consult the convenience of the majority of the profession who prefer to abide by time-honored methods. The pharmacist, retail or wholesale, who is not prepared to carry out the orders of a physician using the official units of the *Pharmacopeia*, is placed on the defensive.

At the session of the Paris Academy of Medicine, held March 16, Chatin mentioned a case of typhoid fever following the ingestion of oysters. He stated that there was little danger of infection from oysters even when grown in unhealthy places if they were placed in pure sea water for a week before being eaten. The sea water hinders the development of the typhoid bacillus and increases the resisting power of the bivalve.

ABSTRACTS.

PRINCIPLES UNDERLYING THE SERUM DIAGNOSIS OF TYPHOID FEVER AND THE METHODS OF ITS APPLICATION.*

As the result of infection with many bacteria or of intoxication with their products, the blood, even when highly diluted, acquires the property of causing loss of motility and clumping together of the specific bacteria concerned in the infection or intoxication. The clumping is called by Gruber agglutination, and is attributed by him to the presence of substances to which he has given the name "agglutinins." He supposes that these agglutinins make the gelatinous capsules of the bacteria swell up and thereby stick the bacteria together. Although there is no proof of this theory, the names "agglutination," to designate the phenomenon, and "agglutinins," for the supposed substances causing it, have been widely adopted.

In the case of motile living bacteria two phenomena characterize the complete reaction—paralysis, or immobilization, of the bacteria, and clumping. Usually these two phenomena go hand in hand, but sometimes there is loss of motility with little clumping or clumping without much cessation of motion; so that the opinion has been expressed that the paralyzing and the agglutinative substances are not identical. The more common deviation from the usual course of the reaction is the occurrence of clumping, with partial preservation of motility.

The agglutinative serum-reaction appears to be of wide although not universal application, both for motile and non-motile pathogenic bacteria, having been demonstrated for typhoid, Asiatic cholera, pneumococcus-infection, tetanus, pyocyanus disease, hog cholera, Malta fever, colon infection, proteus infection, psittacosis, glanders, and several other infections.

The change in the blood upon which the reaction depends is doubtless a spe-

cific one in the same sense as are the antitoxic, lysogenic, and other specific alterations caused by the action of definite bacteria or their products. It is upon this specificity that the diagnostic value of the reaction is based. It is true that, as in the case of the antitoxic, lysogenic, and other protective modifications of the fluids of the body, the normal blood may possess, in some degree, the same property; so that the specific character of the change may not be apparent without resorting to considerable dilution of the blood or serum. Normal blood may agglutinate, to some degree, not only the typhoid bacillus, but various other bacteria. The specificity of this change resulting from infection with a given micro-organism is made apparent by increase of the agglutinative power of the serum only for that micro-organism, or to some extent also for closely allied micro-organisms. The increase of reaction in some degree with closely allied bacteria does not militate against the specificity of the change, for it is only an expression of the natural affinities between varieties and traces of organisms, as between the typhoid bacillus and the bacillus of psittacosis, or between the cholera spirillum and certain other spirilla, or between the varieties of proteus bacilli or of colon bacilli. The development of the specific agglutinative properties of the blood in typhoid fever affords additional proof, if any were needed, that the bacillus typhosus is the specific cause of this disease.

The blood acquires the specific agglutinative power at a variable period, usually within a few days, after the entrance of the pathogenic micro-organism or its products. This power tends to increase, but with much irregularity, during the course of the infection, and gradually to diminish and finally disappear weeks, months, or, it may be, years after recovery from the infection. Widal lays much

* WILLIAM H. WELCH, in *Jour. American Med. Assn.*

emphasis on the reaction being one of infection and not of immunity. Still it is to be noted that, by following procedures for raising experimental immunity to great heights, there may be a corresponding rise of agglutinative power, whereby degrees of this power may be attained which are entirely unknown during natural infections. Thus, Widal, by successive inoculations of an ass, has secured typhoid serum with an agglutinative strength of 1 to 43,000, and Salimbeni has obtained, experimentally, cholera-serum agglutinating in a dilution of 1 to 50,000. Gruber speaks even of immune sera which agglutinated distinctly in a dilution of 1 to 500,000. There is, however, no necessary correspondence between the height of immunity and that of agglutination; especially may the latter lessen or disappear when the former is preserved. In the light of the experimental results, and for other reasons, it seems to the author somewhat misleading to designate the reaction as merely one of the period of infection.

We are not informed as to the nature of the relationship between the agglutinative and the protective properties of the blood. Gruber has based a new theory of immunity on the agglutinative reaction, but this theory is opposed by many facts and cannot be accepted. The agglutinative property has been shown to be distinct from the bactericidal, lysogenic, antitoxic, and other known protective properties of the blood. At present we have no satisfactory evidence that agglutinative reaction is concerned in any of the defensive mechanisms of the body. According to Salimbeni, whose results, however, are not entirely in accord with those of Durham, agglutination of bacteria does not take place within the animal body, although it has been demonstrated that the living blood-plasma possesses the agglutinative property.

We do not know the origin or nature of the so-called specific agglutinating substance, save that in some way it results from the activities of bacteria or their products within the living body. Gruber believes that it is derived from the bodies of the bacteria through the agency of the cells. Bordet considers that it is secreted by leucocytes. Experiments of Widal and Sicard, and of Achard and Bensaude, in-

dicate that it is not secreted by leucocytes outside of the living body, but there is nothing which shows that it may not be formed by cells within the body.

The agglutinative substance is generally believed to be a proteid, as it is precipitated from blood-plasma with fibrinogen and globulin, and from milk with lactoglobulin and casein, but it is possible that it is simply mechanically retained by these albuminous precipitates. In typhoid fever it may be absent from albuminous urine and may be present in urine which gives no reaction for albumin (Widal). It behaves like an albuminous substance as regards filtration and dialysis.

The question whether it is strictly proper to speak of agglutinative, bacteriolytic, antitoxic substances in the blood is a legitimate one. They have never been isolated as chemical substances. Behring has expressed the opinion that isolation of antitoxin will never be accomplished, for, in his opinion, it is a force pertaining to highly-organized material, and there is no more possibility of separating it as a substance than of isolating the magnetic force from an iron magnet. In all probability these various properties, agglutinative, bacteriolytic, antitoxic, belong to the same general category, and we may look upon the agglutinative property also as a physical one which proteid substance may acquire as the result of the activities of bacteria or their products, and no more separable in the form of a chemical substance than is electricity.

The agglutinative property is quite resistant to injurious agencies. It survives desiccation of the blood or serum for months, and may persist for months in blood-serum, even when this is seriously contaminated with micro-organisms. It is not destroyed by sunlight, unless overheated. It is weakened by prolonged heat at 60 degrees C. and is annulled by heating for ten minutes at from 75 degrees to 80 degrees C.

The agglutinative property in typhoid fever is present in a maximum amount in the blood, being somewhat greater in the blood plasma than in the blood-serum. It is found in blister-serum in essentially the same strength as in blood-serum. In other fluids, as the pleural, peritoneal, pericardial, inflammatory, and edema-

tous, it is in smaller and variable amount. In milk and colostrum it is present in marked degree. It is weak and inconstant in the urine, bile, and aqueous humor. It has been found in tears naturally secreted, but is said to be absent from those provoked by irritants. It may be present in typhoid stools (Block). It has not been found in the cerebro-spinal fluid or the fluid in the seminal vesicles. It has not been positively determined whether the distribution of the agglutinative property in the various humors of the body outside of the blood can be explained wholly by processes of filtration and diffusion from the blood-plasma, although these are doubtless the main factors.

The agglutinative reaction may or may not be obtained with the blood of a fetus or new-born infant of a mother with typhoid fever. Chambrelet and Saint-Philippe consider that the presence or absence of the reaction in the fetus depends on whether or not the typhoid bacilli breaks through the placental barrier from mother to fetus and cause infection of the latter. Further investigations are needed to determine this point and thus to decide whether the agglutinative property is passively or actively acquired by the fetus. For it has been shown that this property, like immunity, not only may be actively acquired as the result of infection or intoxication, but may be passively transmitted by injection of agglutinative serum, the reaction in the latter case appearing promptly without symptoms of infection, being relatively slight, and disappearing after a short time.

Courmont thinks that the development of typhoid bacilli in a fluid robs it of agglutinative power. Thus he has found that the vegetation of the bacilli in typhoid serum deprives it, in a few days, of the agglutinative property, and that blood obtained, post-mortem, from the spleen, liver, and mesenteric glands, organs in which the typhoid bacilli are especially abundant, is poorer in agglutinative power than that from other parts. Menetrier found, in a case of typhoid fever, that the pleural exudate, which usually gives the agglutinative reaction, did not do so, and that it contained typhoid bacilli in large number. It has been suggested that these observations may shed light on the exceptional cases of typhoid fever with absence

of the specific serum-reaction. Flexner has shown that, occasionally, the typhoid bacilli develop in the blood in such large numbers as to produce a genuine typhoid septicemia. Still, these observations, interesting as they are and deserving further investigation, must be interpreted with caution, for Widal has found a purulent exudate from an immunized ass swarming with typhoid bacilli to present, even after fifteen months' preservation, an agglutinative power of 1 to 13,000, the power of the blood-serum of the same animal kept for the same length of time being 1 to 14,000.

We have no satisfactory explanation of the production of the phenomenon of agglutination by specific serum. Gruber's explanation, already mentioned, has received no confirmation. The phenomenon occurs with non-motile as well as with motile bacteria, with dead as well as with living organisms. Typhoid bacilli, killed by formol in weak solution, or by heating for five minutes at a temperature of 56 degrees C., are about as sensitive to the reaction as are living bacteria, and retain their agglutinability for a long period. (Widal). The phenomenon, therefore, is a physical rather than a vital one, although probably dependent, in some way, on the protoplasmic constitution of the bacterial cell. Salimbeni, as has been stated, found that the phenomenon does not occur in the living body of immunized animals.

He also found that the presence of atmospheric air greatly favors the reaction, it requiring much more concentrated serum and a longer time to bring about the phenomenon in a vacuum than when the fluid is exposed to the air. Widal has shown that other physical conditions, particularly contact with objects, such as the surface of slide or cover-glass, and partial evaporation, favor the production of the reaction.

Agglutinative serum is often likewise bactericidal and bacteriolytic, but the agglutinative reaction is independent of the bactericidal and is manifest with dilutions of the serum which annul the bactericidal power. In such dilutions the agglutinated and immobilized bacteria are not altered morphologically, or in staining properties, or in pathogenic power, or in any other way, so far as has been determined.

Whether the temporary inhibition of bacterial growth in agglutinative serum is dependent on the agglutinative or on some other property of the serum is not known.

Widal, in his first communication, described both the slow, or macroscopical, and the quick, or microscopical, methods. For each of these he recommended a dilution of one part of blood, or blood-serum, to ten parts of the fluid containing the culture. The macroscopical method consists in adding the blood or serum to be tested either to a young bouillon culture of the typhoid bacillus or to sterile bouillon which is then at once inoculated with the bacillus. In the former case the reaction with typhoid serum appears usually within two or three hours and consists in clarification of the previously turbid fluid, and the formation of a clumpy sediment composed of accumulated bacilli. In the latter case the tube is placed in the incubator, and within fifteen hours the reaction is manifested by growth of the bacilli in the form of a sediment at the bottom of the tube, the fluid remaining nearly or quite clear.

The microscopical test, to which Widal gave the preference, is made by mixing the blood or serum with a young bouillon culture or with a suspension in bouillon or salt solution of a fresh growth of the typhoid bacillus and examining a drop or two of the mixture at once under the microscope. With a dilution of 1 to 10 this microscopical typhoid reaction appears, as a rule, immediately or within a few minutes, and is evidenced by loss of motility and by clumping of the bacilli into masses of various sizes and shapes.

Widal obtained the blood either with a sterilized hypodermic syringe from a vein of the arm or by pricking the finger. It may also be conveniently obtained by pricking the lobule of the ear. A few drops of blood suffice for collecting the necessary amount of serum; indeed a single drop will do for the reaction. The blood may be collected in a small test tube where, usually in a few minutes, it clots. The separation of the serum may be facilitated by passing a sterilized platinum needle between the glass and the clot, or by the centrifuge, or the blood may be collected and allowed to clot in a slanted tube, which is then placed upright, the

separated serum trickling to the bottom.

In a communication made on July 31, 1896, Widal said that results equaling those with blood serum can be obtained with blister serum, and this procedure has been employed with much satisfaction by the Health Department of New York City.

At the same time Widal called attention to the preservation of the agglutinative property in dried blood and serum. Wyatt Johnston deserves the credit of developing the test with dried blood and for introducing the method of serum-diagnosis into the work of the municipal laboratories. The dried blood method, which has been used far more extensively in Canada and this country than in Europe, possesses certain manifest advantages, especially ease of collection, freedom from subsequent contamination, and readiness of transportation, and it has given excellent results in the hands of Johnston and others. The principal objection, and this is of considerable importance when precise results are desired, is the difficulty of obtaining accurate quantitative dilutions with the use of dried blood.

Several observers, including Breuer, Haedke, Du Mesnil de Rochemont, and Scheffer, have expressed the opinion that the macroscopical method is more trustworthy than the microscope. This the author believes to be an error and to be due to unfamiliarity with all of the conditions essential for the accurate employment of the microscopical test. The latter is more delicate, prompt, and precise than the macroscopical reaction, and requires less care in respect to accidental contamination.

A year's experience with the method of serum diagnosis of typhoid fever has led to a general consensus of opinion as to its great value. It has, however, been recognized that certain precautions in the application of the test are necessary in order to avoid mistakes. Numerous modifications of the original methods have been suggested, the most important relating to quantitative determinations. In considering the value and practical utility of such modifications of the test, several points should be borne in mind. Practically all of the methods recom-

mended by competent investigators have given good results in the great majority of cases. For clinical purposes it is desirable that neither the method of obtaining and collecting the blood nor that of conducting the test should be more difficult and complicated than is absolutely necessary. Methods which may be essential for exact scientific work, where every possible source of fallacy is to be avoided, may not be the best for the routine examinations of a clinical or a municipal laboratory. Where absolute accuracy is not obtainable it is, upon the whole, better that the method should err on the side of now and then including a non-typhoid case than in excluding cases of genuine typhoid fever. With due allowance for such considerations as these, we must welcome all efforts to give greater precision to the methods of serum diagnosis and to determine the capabilities of these methods and their possible sources of error. In exact quantitative work with the serum test the most important points to be considered are the characters of the culture, the dilution of the serum, the time limits, the criteria of the reaction, and certain physical conditions influencing the reaction.

There has been considerable difference of opinion as to whether cultures of the typhoid bacillus obtained from different sources are equally sensitive to the agglutinative reaction. Widal, Durham, Stern, and C. Fränkel, who have all had large experience with different cultures, have found only unimportant and inconstant differences in susceptibility to the reaction. The fact determined by Pfeiffer that the less virulent the culture the greater the sensitiveness to the lysogenic reaction (Pfeiffer's phenomenon), seems to have been considered by many, without sufficient investigation, to be equally applicable to the agglutinative reaction. Kolle, without, however, presenting sufficient evidence, emphasizes the greater susceptibility of cultures with weakened virulence to agglutination. The most satisfactory evidence on this point is furnished by Kühnau, who made a careful comparative study of the behavior with the serum test of a non-virulent and a virulent typhoid culture, and found the former to react much more intensely with normal and typhoid sera. He therefore

lays stress on consideration of the virulence of the culture in quantitative work with the serum test. In view of the conflict of opinion further investigations upon this question are needed.

It cannot be doubted that several observers have had to do with typhoid cultures which presented distinct differences in susceptibility to the agglutinative reaction. Especially worthy of consideration, although not wholly in accordance with some results of others, are the observations of Johnston and McTaggart, confirmed by Appel and Thornbury, that solutions of dried blood are more potent than serum in agglutinative power, although not in paralytic effect, and that such solutions from non-typhoid cases are prone to give partial (pseudo-) reactions with frequently transplanted typhoid cultures, whereas this difficulty is largely overcome by using fresh cultures planted from stock cultures a month old. Hence they strongly recommend for the dried-blood method cultures of the latter character. They, as well as other writers, likewise emphasize the importance of considering the composition of the culture medium, which should be favorable to vigorous growth and not too strongly alkaline.

Only young cultures should be used, preferably not over twelve to eighteen hours old, if grown in the incubator. Older room cultures can be used. Old cultures agglutinate more readily than young ones. Either bouillon cultures or suspensions in bouillon from solid cultures may be employed. There is no difficulty in securing uniform suspensions of isolated, actively motile typhoid bacilli, especially from young cultures on dried-out agar. In every case it is of prime importance to make a control examination of a drop from the same part of the culture or suspension which is used for the test and at the time of making the test, in order to be sure that there are no pre-existing clumps, that the bacilli are actively motile, and that the culture is not contaminated.

Stern suggested that the concentration of the suspension—that is, the number of bacilli in it—may be a factor meriting consideration, and Kühnau and Block have shown that this is the case. Weak suspensions are more readily agglu-

tinated and paralyzed than stronger ones. Hence, Kühnau recommends the use of suspensions of known concentration, which can be approximately secured without much difficulty. He uses a suspension in bouillon of a fifteen-hour virulent agar-culture (grown in the incubator) containing about one hundred and twenty million bacteria in a cubic centimetre.

Inasmuch as normal and non-typhoid blood may possess distinct agglutinative property, especial importance is attached to the dilution of the serum, in order to avoid mistaking the normal reaction for one of typhoid fever. The opinion has been widely expressed that the dilution recommended by Widal, 1 to 10, is too low, and that a dilution should be used which is not known ever to give a reaction with non-typhoid blood. The fixation of the upper limit of such a dilution has been placed gradually higher and higher, thus by du Mesnil at 1 to 25, by Kolle 1 to 30, by Grünbaum 1 to 33, by Stern, 1 to 40, by Kühnau 1 to 50. Even if it should be admitted that a reaction in non-typhoid cases with these higher dilutions is ever of such a character as might mislead an experienced observer its occurrence is, according to most observers, very exceptional.

The question arises whether the adoption of a dilution of, say 1 to 50 as the standard, would result in the exclusion of genuine typhoid cases from the diagnosis. Widal divides the typhoid cases in which he has measured the agglutinative power of the blood into five groups: (a) those with very weak power—less than 1 to 100 (four cases); (b) with weak power—between 1 to 100 and 1 to 200 (nine cases); (c) with medium or average power—from 1 to 200 to 1 to 500 (eight cases); (d) with high power—from 1 to 500 to 1 to 2000 (nine cases); and (e) with very intense power—exceeding 1 to 5000 (three cases). In only one case did the agglutinative power not rise over 1 to 40, it being 1 to 30 on the twentieth, and 1 to 40 on the twenty-second day of the disease. In one case Widal found the strength to be 1 to 12,000. In nineteen cases measured by Stern, the agglutinating strength was never less than 1 to 50. C. Fränkel found the average to lie be-

tween 1 to 100 and 1 to 200, sometimes reaching to 1 to 5000. Out of seven cases Kühnau found two in which the serum was active only in dilutions less than 1 to 50, it being 1 to 30 in one, and 1 to 20 in the other case, but in both he made a positive diagnosis of typhoid fever in consequence of disparity of the action of the serum on the colon bacillus and the typhoid bacillus. From the observations thus far reported, although they are insufficient in number for definite conclusions, there would seem to be only small liability of failure to recognize genuine typhoid cases by resorting to dilutions of 1 to 40 or 1 to 50, but unquestionably a few cases would escape recognition, and for this reason lower dilutions should also be used, and if those between 1 to 10 and 1 to 50 give decided reaction there should be, at least, suspicion of typhoid fever.

It is not, therefore, to be recommended that one should make the test with only high dilution, such as 1 to 50. The negative result of a preliminary test with equal parts serum and culture suffices to exclude typhoid reaction. The examination, if positive, may then be made with a low dilution of the serum, and for this Widal's recommendation of 1 to 10 or 1 to 15 may be well adopted. If with this dilution the microscopical reaction is complete and almost immediate, as is often the case, there is practically no risk in making a positive diagnosis. But for absolute certainty and, above all, in cases where the result of the reaction is not prompt, complete, and unmistakable, higher dilutions should be employed; if the amount of serum permits only one such, it may be 1 to 50, but preferably intermediate dilutions should also be made, and it is desirable, if not absolutely necessary, to try dilutions higher than 1 to 50. For making the dilutions there are various simple technical procedures, which involve but little expenditure of time and labor and only small quantities of serum, as, for example, that recommended by C. Fränkel. An accurate fixation of the upper limit of agglutinative power is often tedious and not generally necessary in diagnostic work. A positive diagnosis of typhoid fever based exclusively on the test with a low dilution, in a case which subsequently proves not to

be typhoid in any case, would be a serious mistake.

It is of value to remember that the serum does not always give a positive reaction.

Many cases of typhoid fever are an easy diagnosis.

It is often difficult to make a diagnosis.

be typhoid should not be considered as in any way invalidating the results of an accurate employment of the method of serum diagnosis.

It is self-evident that the employment of varying degrees of dilution of the serum, without at the same time taking into consideration other factors which influence the reaction, has little sense and does not constitute in itself alone an accurate method of mensuration of agglutinative power.

As the rapidity with which the reaction appears and progresses generally varies, other things being equal, according to the agglutinative strength of the blood, it is evident that methods of exact mensuration of the strength must take into consideration the length of time required for the development of the reaction after the addition of the serum. Many writers have not paid much attention to this point. Stern has proposed that a limit of two hours be adopted as an arbitrary standard for the microscopic reaction, and Widal has accepted this proposal. With this unit, an agglutinative power fixed at 1 to 500 means that 1 part of serum added to 500 parts of the fluid containing the culture agglutinates and paralyzes the bacilli within two hours, although a higher dilution may give a decided reaction in six or eight hours. The optimum effect is, according to Stern, not attained before the lapse of six or eight hours.

By varying the time limit, results obtained by lower dilutions may be roughly comparable with those by higher dilutions. Thus, for diagnostic purposes, a fifteen-minute time limit for dilutions of 1 to 10 may be adopted and a two-hour limit for dilutions of 1 to 50 or higher, but it should be understood that in all doubtful cases quantitative determinations by varying the dilution should be employed.

Some writers have proposed to make either the cessation of motility or the clumping the essential criterion of the reaction. Thus Stern selects the clumping and Kühnau the paralysis of motion. In the author's judgment, both phenomena enter equally into the reaction and deserve equal consideration; so that a reaction is not to be considered complete and satisfactory unless the bacilli are

both clumped and rendered immobile. Partial reactions in which one or the other characteristic is lacking may warrant suspicion and lead to further examination, but they should not be made the basis of positive diagnosis. For this reason the use of killed cultures, as suggested by Widal, while it may have a limited field of application, cannot supplant the ordinary method. As already stated, the microscopic reaction is to be preferred to either of the macroscopical methods, although the latter afford striking objects for demonstration. With low dilutions bactericidal and lysogenic phenomena are common, but they do not pertain to the agglutinative reaction itself.

As already mentioned, Salimbeni has shown that free exposure to the air favors the reaction, and Widal has pointed out that partial evaporation at the edge of the cover glass and contact of the specimen with slide and cover glass are also favoring conditions. Hence, the conditions for the reaction are not exactly the same with the serum-bouillon mixture in thin layer beneath the cover glass on an ordinary slide, as in a thick layer, or in a sealed drop culture on a hollow slide, or in a column of fluid in a test tube, or in a moist chamber. The presence of fibrinous masses, granules, and material foreign to blood serum may, perhaps, explain, in part, the greater frequency of partial reactions with normal blood when the dried blood method is used than when the serum method is employed. The temperature of the incubator, by favoring evaporation and in other ways, accelerates the reaction. For exact quantitative work these various physical conditions need consideration and further investigation. Widal prefers the use of ordinary slides to that of hollow ground slides, and does not advise keeping the specimen in the incubator. It is not to be supposed that the diagnostic use of the serum test generally hinges on such delicate points as these, but they are among the points to be considered in the explanation of certain irregularities in the results of the test, in comparing the results of different workers, and in mensuration of agglutinative power, especially with high dilutions.

Statements of different writers as to the occurrence of the agglutination of the

colon bacillus with normal and typhoid sera are not harmonious. Widal and Courmont find that all human sera, whether normal or typhoid, have a slight agglutinating action on the colon bacillus in dilution of 1 to 10, whereas normal sera have only exceptionally any such action on the typhoid bacillus in this dilution. Many observers have noted some agglutination of colon bacilli with typhoid serum, although the reaction is much less intense than with the typhoid bacillus.

Vedel found, in a case with symptoms of typhoid fever but without the typhoid-serum reaction, marked colon reaction, and he interpreted the case as one of colon infection simulating typhoid. He is not, however, inclined to attach much diagnostic importance to the colon reaction, as he found that it might be well marked both with normal and typhoid blood.

Johnston and McTaggart found genuine colon reactions with typhoid blood to be rare, provided the typhoid reaction was well marked. In several cases, however, where the symptoms suggested typhoid but the typhoid-serum reaction was absent, they found marked colon reaction. They are inclined, therefore, to attach diagnostic importance to the latter reaction. The colon cases were mild and of shorter duration than ordinary typhoid fever.

Kühnau makes use of the colon reaction to assist in the diagnosis of typhoid fever in doubtful cases. He finds that normal serum reacts in the same way with both colon and typhoid bacilli, whereas typhoid serum, even when of weak specific power, reacts much more intensely with the typhoid bacillus than with the colon bacillus. By availing himself of this unequal action of typhoid serum upon the two species of bacteria, he felt justified in making the diagnosis of typhoid fever when the agglutinative power of the serum did not exceed 1 to 20. Kühnau's suggestion is interesting, but further investigations are needed to determine its value.

We must await further studies before Johnston and McTaggart's highly suggestive views as to the existence of colon infections simulating typhoid and cap-

able of diagnosis by the serum reaction with the colon bacillus can be accepted.

As was first pointed out by me in 1890, the colon bacillus is an extremely common secondary invader of the body in all sorts of conditions, particularly those with lesions of the intestine. It can very frequently be found in internal organs outside of the intestine in typhoid fever, if careful search is made. We have no satisfactory proof that it produces either symptoms or lesions in most of these cases, and one would expect more common and intense serum reactions with bacillus coli in typhoid fever, if the organism was engaged in pathogenic work. The writer has repeatedly taken occasion to protest against what seem to him unwarranted inferences as to the pathogenic significance of the mere detection of the colon bacillus in the internal organs at autopsies, although there can be no question that under certain conditions this bacillus may be pathogenic for man.

As the colon group of bacilli contains numerous races, some approaching the typhoid bacillus closely, it is to be expected that they will vary markedly in their sensitiveness to agglutination with different sera.

Durham found that typhoid immune serum in no instance produced any agglutinative reaction with ten specimens of the bacillus coli obtained from various sources. Colon immune serum reacted on its own race of bacilli exactly like typhoid serum on typhoid bacilli, but it did not react with all races of colon bacilli, a graduated series of effects being observed with different specimens of these bacilli. Rodet, however, whose duties are reported in much less detail than those of Durham, found a certain degree of reciprocal action between colon and typhoid immune sera and their respective bacteria.

The presence of the specific agglutinative reaction can usually be counted on by the end of the first or the beginning of the second week of typhoid fever. It may appear as early as the second day of the disease (Johnston and McTaggart, C. Fränkel), but this is very exceptional. It may here be noted that the determination of the exact day of a disease, often so gradual and insidious in its development as typhoid fever, must frequently be more

or less arbitrary, and will vary according to the case and with different observers. Sometimes the first appearance of the reaction is delayed, exceptionally until the end of the second or into the third week, or even later. There are cases in which the reaction is missed during the first attack and makes its appearance in the relapse (Breuer, Thoinot, Biggs, Park, and others). It has been missed until the first days of convalescence. Blumenthal relates an interesting case in which the reaction was absent during the fever, tests being made on the twelfth and twenty-first days with serum dilutions of 1 to 10, but it was found with dilutions of 1 to 100 two days after the beginning of apyrexia. Achard likewise once found the reaction only during convalescence. We have not at present a sufficient number of accurate data to furnish definite figures as to the frequency of these delayed reactions, but their occurrence undoubtedly constitutes a defect in the method of serum diagnosis of some importance. A negative result of the test does not exclude the diagnosis of typhoid fever. The probability against this diagnosis is the greater, the later the period of the fever in which the negative result is obtained and the oftener the examinations are repeated. As regards the interpretation of negative reactions, the serum test does not differ from other bacteriological diagnostic tests—that for the tubercle bacillus, for instance.

There are authentic cases of typhoid fever in which repeated examinations of the blood during the course of the disease and its convalescence failed to reveal the specific agglutinative reaction, even with serum dilutions of 1 to 10. We cannot, at present, say what percentage of the total number they make. Widal and Sicard found absence of the reaction in only 1 out of 163 cases of typhoid fever examined by them. In this negative case, in which the diagnosis was confirmed by cultivation of typhoid bacilli obtained by hypodermic puncture of the spleen, the reaction was absent during the fever, the apyrexia, the relapse, and the convalescence.

Of 116 cases of typhoid fever examined by Courmont, the reaction appeared in all, being delayed after the eighth day in only five. Of 70 cases ex-

amined by Chantemesse, it was present in all. Of 129 cases examined by Johnston and McTaggart, if a few cases examined only in convalescence or at a very early stage without re-examination be excluded, the reaction was missed in only 1. In many reports cases believed to be typhoid are recorded as giving negative reaction when only one examination was made, this being sometimes early in the disease. Such cases doubtless belong mainly to a group with delayed reaction. The importance of repeated examinations is illustrated by such observations as Stern's, in which the test was negative at the end of the second week and positive two days later; of Widal's negative on the tenth, positive on the twenty-second day, and several others of similar purport.

The agglutinative power of the blood tends to increase during the progress of the fever, but there are exceptions, and, in general, the intensity of the reaction is subject to irregularities and oscillations which may be notable from day to day. There may be marked sudden rise or fall of reactive power. While weak reactions are more common in mild cases, there is no definite correlation between premature or delayed development or the intensity of the reaction and the gravity of the disease. The persistence of high agglutinative power—for example, 1 to 2000—after subsidence of the fever, does not prevent relapses.

In the majority of cases the specific agglutinative power of the blood diminishes in the first weeks or months after cessation of the fever and disappears within a year. Exceptionally it may vanish as early as eight or ten days after the fever. Widal and Sicard noted its appearance on the eighteenth and twenty-fourth days, Breuer on the seventeenth and twenty-fifth days, E. Fränkel on the twenty-eighth day after defervescence, etc. Disappearances at such early dates as these are, however, not the rule. According to Courmont's experience, the serum reaction disappears in children most frequently during the course of the first two months, and in adults toward the fifth and six months, although it is not uncommon for it to continue a year. The specific reaction may, however, persist for years, perhaps indefinitely. Of

40 cases which had had typhoid fever at least a year before examination, Widal and Sicard found the agglutinative reaction, either marked or slight, in 11; after one and one-half years, 1 case, reaction weak; after two years, 1 case, reaction weak; after three years, 2 cases, in one marked in the other weak; after six years, 1 case, reaction 1 to 10; after seven years, 1 case, reaction marked; after eight years, 1 case, reaction 1 to 1800; after nine years, 3 cases, one marked, 1, 1 to 40, and 1, 1 to 30; after twenty-six years, 1 case, 1 to 30. Kühnau found after one year, 2 cases with reaction of 1 to 80; after two years, 1 case, reaction 1 to 60; after seven years, 1 case, reaction 1 to 60.

All others of a series examined (total number not stated) showed negative reaction after one year. It was observed by Widal and Sicard that, in contrast to the reactions during infection and for the first weeks after defervescence, those of long standing showed no notable fluctuations in intensity during the periods of examination, extending sometimes over several weeks.

The persistence of the specific reaction after typhoid fever is of importance from two points of view: retrospective diagnosis and interpretation of the diagnostic significance of the reaction during a febrile infection. It is sometimes of interest and practical importance to determine that an individual has previously had typhoid fever. Thus Courmont was able, by the serum diagnosis, to determine that a patient with multiple neuritis, supposed to follow an attack of dysentery, was in reality convalescent for a month and a half from typhoid fever, and Achard recognized the real nature of an attack of osteomyelitis in a patient who had typhoid fever a year before (cited from Widal and Sicard).

Many writers have called attention to the evident possibility of a mistake in diagnosis when the serum reaction is found in a person with a febrile infection, who has recovered from typhoid fever, although it does not appear that any serious difficulty has been encountered thus far from this source of error. It, however, indicates the importance of obtaining a careful history of the patient, not only as regards recognized typhoid fever,

but as to attacks interpreted as dysentery, gastric fever, appendicitis, malaria, etc. An observation reported by Stern indicates that the specific typhoid reaction may be acquired even without manifest illness. He suggests that careful quantitative estimations of agglutinative power may restrict the possibility of error in diagnosis arising from long-persistent reactions, as increase or diminution in the course of the fever or of the convalescence would speak for fresh infection. Even if the fullest possible allowance be made for this source of error, it applies to so small a number of cases that the value of the method is not seriously impaired.

The blood of many hundred persons, either healthy or affected with diseases other than typhoid, has been tested for the specific typhoid reaction, and it can now be asserted that a serum reaction, which an experienced observer using accurate methods would consider characteristic of typhoid fever, is to be found most exceptionally in those who have not had typhoid infection.

Several observers, especially the Germans, find that by adherence to Widal's original directions mistakes may occur, but that these can be avoided by attention to quantitative determinations, especially dilution of the serum, time limits, and characters of the culture used for the test. The most remarkable observations on this point come from Breslau, and are reported by Stern and Kühnau. Stern examined the blood serum of 70 persons not suffering from typhoid fever, and, according to their statements, never having had typhoid fever. In 20 of these the serum had an agglutinative strength of 1 to 10, in 5 a strength of 1 to 20, and in 2 a trace of reaction was obtained with dilutions of 1 to 30. In none of these cases did he find any reaction with dilutions of 1 to 40. Of more than 50 similar cases examined by Kühnau, in 41 no reaction was obtained with dilutions higher than 1 to 5. In 8 agglutination was obtained with dilutions of 1 to 10, up to 1 to 20; in 4 with 1 to 30; in 3, 1 to 35, up to 1 to 40; and in 1 even with 1 to 50.

In judging these results, apparently so divergent from those of Widal and nearly all others, it is to be noted that both

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Stern and Kühnau used the microscopic reaction, made two hours the time limit, even for the lowest dilutions, kept the specimens in the thermostat, and that Stern regarded the clumping and Kühnau the paralysis of motion as the criterion of the reaction, thus apparently recording as genuine what many others would consider partial or pseudo-reactions. It is clear that those who interpret only complete reactions occurring within fifteen to thirty minutes at room-temperature as genuine reactions would not be likely to obtain any such results as those reported by Stern and Kühnau. Nevertheless, it must be admitted that even partial and late reactions are unwelcome and disturbing, even if an observer thinks that his skill and experience will enable him to avoid mistakes from the occurrence. It may also be, as suggested by C. Fränkel, that Stern's and Kühnau's normal and non-typhoid cases include not a few who had recovered from unrecognized typhoid infection, and this supposition is the more probable in consequence of the prevalence of typhoid fever in Breslau. Stern himself calls attention to the liability of failure to recognize certain mild cases.

There have been a few cases reported in which the diagnosis of typhoid fever was made on the basis of the serum reaction, but which the authors, from subsequent developments, considered to be free from typhoid infection. It has been claimed, therefore, that positive serum reactions are not a sure sign of typhoid fever, although no one claims that the chances of error from this source are more than slight. The cases reported by Achard and Bensaude, Jez, Ferrand, du Mesnil, and Van Ordt have given rise to the most discussion. For what seem to the author justifiable criticisms of these reports, he would refer especially to the papers of Widal and of Stern, and limits his remarks to some general statements concerning such alleged failures of the serum tests.

In the first place, as the matter now stands, positive reactions obtainable only with dilutions lower than 1 to 50, possibly than 1 to 60, especially if the reaction is partial and late in appearance, are not certain diagnostic signs of typhoid

fever. In most of the cases just referred to no exact quantitative estimations of the agglutinative strength of the serum were made, and hitherto it has not been shown that the reaction ever occurs with non-typhoid serum in dilutions exceeding 1 to 50, with observance of other quantitative points which he has already considered. With the limited number of observations, however, which we now possess, we cannot, of course, say but that such cases will be found in the future.

In the second place, infection with the typhoid bacillus cannot be positively excluded either on clinical grounds alone or by anatomical examinations at the post-mortem table. Infections with the typhoid bacillus occur without any characteristic anatomical lesions. There may be entire absence of ulcers or other lesions of the intestine. There has recently been, at the Johns Hopkins Hospital, a case with positive serum reaction, from which Dr. Flexner cultivated, in large number, typical typhoid bacilli from the gall-bladder, although there was no previous history of typhoid fever, and there were no intestinal lesions. In 1891 the author called attention to the favorable conditions offered by the bile for the prolonged survival of the typhoid bacillus. In one case the author was able to demonstrate large numbers of typhoid bacilli in the bile of a rabbit, one hundred and twenty-eight days after intravenous injection of 0.5 c.c. of a bouillon culture. Pick has reported a case with marked positive serum reaction in which at the autopsy no typhoid intestinal lesions and no swelling of the spleen were found, but bacteriological examination showed the presence of typhoid bacilli, not, however, in the spleen. Guinon and Meunier's case is instructive. During life the symptoms were those of acute miliary tuberculosis and typhoid fever combined. Serum reaction was positive. At autopsy the lesions appeared to be only those of acute miliary tuberculosis, small ulcers in the intestine being typically tubercular in aspect. Typhoid bacilli, however, were cultivated from the spleen and other parts. As both the symptoms and the bacteriological examination indicated that the typhoid infection was in course of disappearance, the case, if examined at a

somewhat later period, might readily, as Guinon and Meunier remark, be placed to the discredit of the positive value of the serum test. We are justified, in the light of such cases as these, in demanding that thorough bacteriological examinations be made before cases which have given, during life, the characteristic serum reaction, but which do not present, at autopsy the anatomical lesions of typhoid fever, be recorded as free from infection with the typhoid bacillus.

In the third place, the difficulty of excluding a previous attack of typhoid fever, after which, as already stated, the specific serum reaction may exist for years, is to be borne in mind.

In conclusion, the author would emphasize the following practical points:

1. Experience has demonstrated that the method of serum diagnosis of typhoid fever is of great practical value.
2. The alteration of the blood on which this method is based is a specific effect of infection or intoxication with the typhoid bacillus.
3. The microscopical serum test is to be preferred to the macroscopical methods.
4. Quantitative determinations, relat-

ing especially to the culture, the time limits, and the dilution of the serum, are of importance, and, at least in doubtful cases, should not be neglected.

5. As the reaction may be delayed or occasionally absent, a negative result of the test does not exclude the diagnosis of typhoid fever. The later in the course of the disease the test is applied, and the oftener the examinations are repeated at intervals, the less is the probability of the existence of typhoid fever.

6. The persistence of the reaction, sometimes for years, after recovery from typhoid fever, is to be borne in mind in interpreting the reaction in febrile conditions. The appearance of the reaction and its increase during the period of observation speak for fresh typhoid infection.

7. The danger of mistakes from positive reactions in non-typhoid cases can be guarded against in nearly all cases.

8. Provision should be made, especially by the establishment and support of municipal or State laboratories, to render generally available to practitioners the serum method of diagnosis, as well as other bacteriological procedures of similar practical value.

SOCIETY REPORTS.

AMERICAN PUBLIC HEALTH ASSOCIATION.

The American Public Health Association held its quarter-centenary in this city the 26th, 27th and 28th ult. Many of the Southern members were prevented from attending by reason of the prevalence of yellow fever. Resolutions of regret for the resignation of the Secretary, Dr. Irving A. Watson, of New Hampshire, and of appreciation of his services, were passed.

Dr. Frederick Montizambert, General Superintendent of Quarantine of the Dominion of Canada, reported for the Committee on Steamship and Steamboat Sanitation. Among other points touched upon were the necessity of free exposure of bedding to sunlight and air, and the unhealthfulness of having dining-rooms below the water-line rendering proper cleanliness and ventilation difficult. Much attention was given to the matter of spitting as a means for spreading the contagion of tuberculosis. All spittoons provided should contain a disinfecting solution which should be fre-

quently changed. Much hope was expressed that the public would soon become educated to recognize the expediency of controlling such means of infection.

A paper entitled "A Contribution to the Study of Yellow Fever from a Medico-Geographical Point of View," prepared by Dr. Eduardo Liceaga, President of the Supreme Board of Health of Mexico, and Dr. José Ramirez, Secretary of that Board, excited general interest, and was followed by a discussion. The paper said, in part:

We have thoroughly proved that no source of yellow fever exists on the Pacific Coast of the Mexican Republic; therefore, on receiving notice that fatal cases had presented themselves of a disease which was supposed to be yellow fever, the Board sent to the port of Manzanillo one of its delegates, who through his own scientific knowledge, as well as through his long residence in the port of Vera Cruz, has obtained a perfect acquaintance with the symptoms of yellow

fever. He made a careful study of the last cases that presented themselves of that epidemic disease. This careful investigation showed him that what was suspected to be yellow fever was no such thing. It was probably a paludic fever of hemorrhagic type, which, through the exceptional conditions brought about by the putrefaction of a large quantity of dead fish around the town, had assumed something of a typhus form.

There is great difficulty in establishing an exact diagnosis between yellow fever and certain forms of paludic infection that in hot countries assume a very similar aspect and the serious character that is generally borne by that disease, so much so that even persons who are accustomed to observe the two affections are often uncertain as to the diagnosis. These doubts will in future be dissipated, if, as is to be hoped, the discoveries of Dr. J. Sanarelli are confirmed, who appears to have discovered the yellow fever microbe in the city of Montevideo. A knowledge of the cause of the disease will not only allow us to establish an exact diagnosis, but will also doubtless facilitate the means for attacking it in a certain manner.

The epidemic of yellow fever that is now prevalent in some parts of the coast in the United States has now presented a phenomenon that is unique up to this date. Our port of Vera Cruz, which three months ago had not suffered from yellow fever, is now taking serious precautions to avoid contagion from the United States.

DR. WILLIAM BAILEY, of Louisville, in discussing the paper, said that on August 14, a man came to Louisville from Ocean Springs, where the epidemic is thought to have started, displaying marked symptoms of yellow fever. He subsequently died in a private sanitarium after having shown every symptom of yellow fever. Notice was sent to the authorities at New Orleans, and the latter were advised to investigate the disease at Ocean Springs. This was done; but, in the face of the positive diagnosis of the man who died at Louisville, the disease at Ocean Springs was decided not to be yellow fever. A week later new cases developed in Ocean Springs, and the epidemic was recognized.

DR. P. H. BAILHACHE, of the United States Marine Hospital Service, thought a resolution calling on Congress to appropriate an abundance of money for a continued examination of yellow fever was the only way to get over the trouble. He said it was generally believed that Consuls do not report enough concerning the disease. They do not depend on personal inspection of cases, he said, but on the reports of the Health Boards.

DR. RAYMOND said that Sanarelli's claims to have discovered the bacillus of yellow fever had not yet been substantiated, and that it was probable that it was the same discovery made by Surgeon-General Sternberg.

J. V. HUGHES, of Montreal, reported for the Committee on Sanitation, with special

reference to drainage, plumbing and ventilation of public and private buildings. Among other things he said: It is no more possible to lay down a rule covering every possible contingency called for in scientific plumbing and ventilating, than it is to apply fixed rules to the practice of medicine. If it were, there would be little need of skilled physicians. The principle of scientific medicine and plumbing are fixed, but the application of these principles calls for the intelligence acquired by education and developed by practice. Much injury has been done by attempting to frame plumbing by-laws that will apply to every case. The conditions vary in almost every building, and to adapt the principles to the special requirements requires not only practical but scientific knowledge. If the American Public Health Association would make a study of the general principles covering this question and embody them in a code having its endorsement, leaving the practical application of those principles to those whose special duty it is to apply them, much good would result.

DR. JOHN L. SEAL, of Paterson, N. J., said: Every building intended for occupancy should have some special means of ventilation. These means should be commensurate with the requirements of any particular building. The more simple and uncomplicated the means used the more successful is likely to be their operation.

The object of plumbing is, first, immediate withdrawal from a building of certain waste products of life, the presence of which would prove detrimental to health; and second, to prevent harm to the inmates of the said building arising from these products after removal, through the means of their removal. Not only are we threatened by the presence of these substances, but we are also threatened by certain dangers inherent in the best systems at our disposal for ridding ourselves of them.

DR. E. A. ABBOTT, of Philadelphia, objected to the interpretations of the conditions to the effect that modern teaching on ventilation, as formulated from exact examinations, does not justify the belief that there is present in improperly ventilated houses substances capable of inducing specific diseases, but that the evil results of bad ventilation are principally seen in the personal discomfort to which they give rise.

Unless the plumbing is so defective as to permit of the leakage into the house of actual infective matters contained in the examinations, it is impossible by any of the approved scientific methods to demonstrate in the air of otherwise badly plumbed houses matters that can be certainly said to stand in causal relation to disease. The result of experiments on the relation between the gaseous products of decomposition and health have failed to demonstrate that the former stands in any definite causal relation to the latter. Statistics show further, that the health of workers in sewers, upon drainage fields, and those em-

ployed in and about glue factories is, in general, as good as that of the community at large.

The annual address of the President, Dr. Horlbeck (see page 584) dealt mainly with the progress made on general sanitary lines.

A resolution was offered looking to the construction of a "National Board of Health, made up of delegates from the respective State Boards," and the same was adopted.

Resolution offered calling for a better regulation of public fountains, and the doing away of the public use of drinking cups.

Discussion continued on papers "On Fumigation Experiments with Formaldehyd," by Prof. Severance Burrage, S. B., Lafayette, Ind., and "On the Determination of the Amount of Formaldehyd Yielded by the Formaldehyd Lamp," E. A. de Schweinitz, Chief of the Bio-Chemie Division of the United States Department of Agriculture, Washington, D. C.

DR. ROHE, Baltimore: The subjects discussed in the papers read yesterday on disinfectants, is too important to be dropped without further comment. I am accustomed to tell my students that the spread of contagion in typhoid fever is due to the neglect of proper precautions. The instructions laid down last year by the committee having the subject in charge would be effective if properly carried out. In the Insane Hospital, with the use of improved methods in antisepsis, the mortality from tuberculosis within a few years has been reduced from 20 to 6 per cent. As regards yellow fever, and the precautions to be taken for prevention of contagion, I can say little except to note that often those who have the most to say on that subject are resident some five hundred miles from point of contact.

DR. GIHON: As regards some of the quarantine restrictions for the prevention of infection, they may become brutal in the extreme, and I desire to enter protest. I recall the case of a sensitive and refined girl with typhoid fever, who was removed from ship board to a private hospital, and to whom her father and mother were denied access. She died in eight days without having the comfort of her mother's presence during her last hours. That is not the kind of quarantine that is going to prevent typhoid fever.

DR. PETER BRYCE, Ottawa, Can.: It would be well to know all the facts of this case before passing judgment. There may have been good reason why the presence of any one in the sick room would have been objectionable.

DR. J. E. MONJARIS, San Luis, Potosi: The sanitary precautions necessary to be used in infectious diseases depends much upon the nature of the disease. Restrictions which would be right and proper in yellow fever would be brutal in tuberculosis or typhoid. In the latter diseases it is only necessary to isolate and render inert the sputa and de-

jecta; in the former we must isolate the patient.

DR. STERNBERG spoke briefly upon the necessity for a National Health Service, assisted by the co-operation of State Boards. The matter came before the committee two years since at the meeting in Mexico, and last year at Chicago. "We need a sanitary service for the whole country. The head of this department should be appointed by experts, and the position should be permanent. When the great body of the profession unite on some specific plan for the construction of such service, the final accomplishment cannot be long deferred."

His report was referred to Executive Committee.

Recent Experiments Relating to the Etiology of Yellow Fever,
by George M. Sternberg, M. D., Surgeon-General U. S. Army.

(See page 582.)

DISCUSSION.

DR. HENRY B. HORLBECK, Charleston, S. C.: I have been much interested in the remarks made by Dr. Sternberg, a leader in bacteriology. He is on a "hot scent," but as in the case of the hunter and hounds, the *corpus delicti* has not yet been produced, and there may be many baffling obstacles before the object is reached. I trust there will be an appropriation made for the continuation of the work, and that there may be a permanent laboratory fitted up in the Island of Cuba, where the great work for humanity can be pursued under the most favorable circumstances possible. There are in this country ten or twelve millions of people who, one might say, were living over a powder magazine. In 1888 an incident with which I was personally conversant happened at Key West, and which emphasizes most forcibly the horrible results which this scourge is capable of inflicting. At a hotel in this town arrived a couple requiring accommodation for the night. The hotel was crowded, every bed occupied. A mattress was prepared for the couple and they retired to rest, ignorant of the terrible fate awaiting them. Within twenty-four hours they were both dead, and investigation revealed the fact that this *mattress* had been shipped from Cuba, and had been used in an infected house before shipment. We know almost nothing about yellow fever. We don't know how the infection occurs; it is all guess work.

A physician from St. Louis: I would like to ask Dr. Sternberg whether anything has been done to demonstrate by tests, the presence of this bacillus in suspected cases? Is there any practical test for the blood, as in the malarial or typhoid invasion? A case came under my attention in St. Louis recently which presented some suspicious symptoms. There was a sub-icteroid hue of the skin, temperature up, and pulse down. Examination of the blood revealed

the plasmodium malaria. The patient was put on quinin and the case cleared up at once.

DR. E. O. SHAKESPEARE: The indications revealed in autopsies seem to indicate that the *materies morbi* finds its selective point, or is first centered in the alimentary canal, and this would seem to prove the entrance of the infectious element by the mouth and stomach.

A physician from Kentucky: In the cases I have seen, the kidney seemed to have received the brunt of the attack.

DR. SHAKESPEARE: I admit that the gravest lesions are found in the kidneys, liver and spleen, and as these organs are in direct connection with the alimentary canal through their blood supply, the presumption would be still in favor of my theory.

DR. ———: If the water supply could be accused of carrying infection, the disease would at once spread throughout an infected community. There can be little doubt but that in yellow fever, as in cholera, typhoid, etc., flies exercise to a large extent the function of carriers of the infective poison. The poisonous agent in yellow fever may probably be found in the excreta. Destroy the excreta and you destroy the nidus; destroy the nidus and you destroy the yellow fever.

The association determined to appoint five members of a committee to investigate the causes of yellow fever. Alderman Black, of Ottawa, extended an invitation to the association to meet there in 1899.

Of special interest was the report on the new disinfectant, formaldehyd. The outcome of the discussion was that this disinfectant is probably the best in existence, but that the difficulty of generating formaldehyd gas makes its extended use unlikely.

The Dangers of the Barber Shop as discussed by DR. SUITER, also aroused the interest of the audience.

There is scarcely a step in the processes usually employed by the barber to-day which does not carry an element of danger when the most careful precautions are not observed. It is remarkable that the first steps toward antiseptic methods in this trade were introduced only last month at Paris. There metal combs and brushes are required, and they, with the razor, clippers and scissors, are subjected to heat of 100 degrees centigrade before and after use. Shaving brushes are washed in boiling water. The towels are sterilized, the barber must wash his hands in an antiseptic solution and the hair which is clipped must be removed and destroyed at once.

The reason why I grew a beard, was that a patient I was treating for an organic disease which had broken out in eruptions on his face had just gotten up from a chair in a barber shop which I frequented. The alum sticks to staunch the flow of blood and the block of magnesia for drying the newly-shaved face are used for customer after customer, and should be, like the articles of the trade, made subject to restriction.

Professor Edward Jackson, of the Philadelphia Polyclinic Hospital, told of **Eye-strain from Poor Window-glass**.

Inequalities in the thickness of a pane of glass make the pane act on the eyes looking through it like a lens. The glass is then distinctly part of the eye. When the eye wanders over a pane that is unequal, the muscles are unable to adjust themselves to their quickly varying conditions, and eye strain is the result. Car sickness, that well-known feeling of nausea which comes in a long journey on the railroad, is due in most cases to this eye-strain. Railroad companies should by all means put plate glass of moderate thickness in their car windows, for that glass only is free from inequalities.

DR. ROBERT L. PITFIELD, assistant bacteriologist of the Pennsylvania State Board of Health, told of some of the filthy places in which vaccine virus was made. Two places in Chambersburg were reported to be especially dirty, and the virus for vaccination from these places was often no more than pus containing deadly germs of tetanus and other diseases. Arm swellings, severe vaccination marks, erysipelas and other troubles arising from vaccination are due to impure virus.

DR. REYNOLDS, of Chicago, who discussed the paper, said that it was undoubtedly true that the opponents of vaccination, if the present impurity of virus continued, had grounds for opposing the practice. Official control of vaccine virus plants was recommended. Glycerinated lymph, kept ten days, was free from all pathogenic germs.

DR. EMIL A. DE SCHWEINITZ, of Washington, D. C., Chief of the Bio-chemic Division of the United States Department of Agriculture, was appointed a delegate to the Congress on Tuberculosis, to be held next year in Paris.

A resolution presented by DR. HOWARD H. ANDERS, and referred to the Executive Committee, was as follows:

"Whereas, The common and promiscuous public use of drinking vessels, under existing conditions and well-established hygienic principles, is positively and essentially unclean and unsanitary, and therefore a menace to the public health; and

"Whereas, During the past three years many churches of various denominations have recognized the need of prophylaxis in administering the communion wine by adopting individual communion cups or chalices, as being cleaner and safer, at the same time not less satisfactory and sacred as substitutes for the communion cups; be it hereby

"Resolved, That the American Public Health Association, in endorsing the individual communion cup reform, approves cordially the action of the churches, and recommends earnestly the adoption, for sanitary reasons, of individual communion cups wherever communion cups are now in use; and further, be it

"Resolved, That this association recom-

mends still more emphatically and urgently to the public the general use of individual cups, particularly in schools, on railway trains, in stations, at public fountains and the like, as contributory to the better preservation of the public health and the prevention of communicable diseases."

DR. SAMUEL W. ABBOTT, Secretary of the State Board of Health of Massachusetts, read a paper entitled

Consumption—An Indoor Disease.

(See page 586.)

DR. S. A. KNOFF, of New York, read a paper on the

Urgent Need of Hospitals for the Consumptive Poor.

(See page 587.)

He was followed by DR. MAZYCK P. RAVENEL, with a paper on

Tuberculosis and the Milk Supply.

(See page 588.)

DISCUSSION.

DR. LAURENCE FLICK: This very important subject cannot be too much discussed. We all agree that the disease is preventable, and that the two most prolific sources of contracting it is through the sputum, and from domestic cattle.

I do not accept the idea that infection was so much conveyed through milk. I have not seen the first well-established case. There is a close clinical relation between persons in the same family. Cases where children contracted tuberculosis usually occurred in the same house where their parents having the disease resided, and, therefore, only by the establishment of special hospitals could the diseased persons be removed from the family centre. Wherever cases had been taken from the family circle there had been a reduction in tuberculosis. No hope of accomplishing great results could be looked for until the public was educated on the subject of the infectiousness of the disease. This education was going on, however, and in Philadelphia in the last ten years there had been a reduction of the disease by one-third. In

Pennsylvania we are trying to educate the people to get a hospital, and in twenty-five years we will succeed in practically stamping out the disease here.

DR. LYDIA RABINOWITSCH, of the Woman's Medical College, was accorded the privilege of taking part in the discussion, and told of experiments which she had made in the past two years with butter and milk. In the milk she had found from 20 to 30 per cent. of tuberculous bacilli. In eighteen samples of butter from different stores she had not found one bacillus of tuberculosis, but had found a micro-organism which resembled very much the bacillus of tuberculosis.

The discussion of the matter of pure drinking water followed, and these papers were read:

DR. E. T. STEWART, of East Orange, N. J., on a "Plan of Purifying Water."

F. H. NEWELL, Hydrographer of the United States Geological Survey, Washington, D. C., on "Investigations of Water Supply by the United States Geological Survey."

GEORGE E. WARING, JR., Commissioner of the Department of Street Cleaning, New York, "From Sewage to Drinking Water: A Study of the Natural Process of Sewage-Purification, and the Means Whereby Their Efficiency Can be Increased."

"Hydrants—Closed Fountains," by DR. Emilio Zertuche, Delegate from the State of Pueblo, Mexico; "Typhoid Infection," DR. Henry M. Bracken, Secretary of the State Board of Health of Minnesota; "A Brief Review of the Work of the Sand-filters at Lawrence, Mass.," by H. W. Clark, chemist in charge of the Lawrence Experiment Station. (See page 586.)

These officers were elected: Professor Charles A. Lindsley, M. D., of Yale University, President; DR. Benjamin Lee, Philadelphia, First Vice President; DR. John C. Shrader, of Iowa City, Iowa, Second Vice President; DR. C. O. Probst, Columbus, Ohio, Secretary; DR. Henry C. Holton, Brattleboro, Vt., Treasurer; and DR. George H. Rohe, Sykesville, Md., Professor Franklin C. Robinson, Brunswick, Maine, and DR. Peter H. Bryce, Toronto, Ontario, Executive Committee.

PERISCOPE.

DR. BENJAMIN LEE, Secretary of the State Board of Health of Pennsylvania, has characterized the prevailing custom of introducing a water supply before constructing a scientific system of sewerage as placing the cart before the horse.

England saves the lives of 20,000 of her inhabitants yearly, since she began to maintain special hospitals for her dependent class of consumptives.

Medical directors of life insurance companies declare that after the age of forty the mortality among beer drinkers is three times as great as that among total abstainers.

According to the *London Lancet*, of nineteen physicians who were superintendents of Keeley cure establishments, ten have entered hospitals for treatment where no specifics are used.

The Prognostic Stage of Tuberculosis.—Dr. A. Mansfield Holmes, of Denver, Colo., believes in the possible estimation of the recuperative power of tuberculosis. Out of 100 cases carefully studied by him he claims to have elaborated the law of disintegration as being the same in the leucocytes as in the system as a whole. He prepares the blood films, fixes, stains and mounts them. The finger of the patient antiseptically cleansed he pricks with a sterilized needle, uses the blood drop with due celerity, and scrutinizes the shade communicated by the staining solution to the cell tissues. The leucocytes, he maintains, forbode the doom by showing the earliest signs of disintegration, and thereby giving the greatest chances for recovery, by perhaps the abortive method of acclimation or of rational nutrition.—*Jour. A. M. A.*

Lesser (*Berl. klin. Woch.*) divides unmerited syphilis into (1) congenital syphilis, (2) syphilis contracted in legitimate sexual intercourse; and (3) syphilis contracted by direct or indirect contact with the syphilis virus exclusive of sexual intercourse. It is the last-named group which Lesser chiefly describes here. Of course every extra-genital chancre must not be placed in this group. The author maintains that in a large number of cases of syphilis apparently contracted in shaving, a wound has been subsequently inoculated by kissing. The last group of unmerited syphilis may be divided into that conveyed (1) by direct personal contact; (2) by indirect means; and (3) by and to medical men in their professional work. In the first group are included cases in which syphilis is conveyed from one child to another or from a child to an adult. Here kissing is the most important cause, but syphilis due to suckling also falls under this heading. Many objects may be the means of indirectly conveying syphilis, such as those used for eating and drinking purposes. On the tonsil it is not necessary to have a breach of surface for the infection to take place. In the third sub-group the medical man may be the means of conveying the disease, as in various operations, as by inoculation and injection, by catheterization of the Eustachian tube, by the use of caustics, etc. Medical men or nurses may contract the disease themselves as in syphilis technica. There are other cases in which the cause of the infection cannot be ascertained. The diagnosis in unmerited syphilis may be very difficult; in the author's opinion the extra-genital primary lesion is in the majority of cases overlooked. Again, the non-recognition of the disease may lead to its further transference. Patients with ordinary syphilis mostly know the disease and are more or less careful not to convey it to others. Of course the disease is originally derived from an ordinary case, but unmerited syphilis may under conditions lose the character of a disease of the generative organs. Not

withstanding that the number of cases of syphilis contracted in the ordinary way far exceed those of unmerited syphilis, yet something can be accomplished in prophylaxis by speedy diagnosis and treatment, and especially by preventing the further spread of unmerited syphilis.—*Exchange.*

The suicides from carbolic acid continually reported from all parts of the country simply constitute a public scandal. Why should this poison still be sold right and left, like milk or butter, to anyone who chooses to go to a chemist or oilman and ask for it? For years general newspapers have denounced the practice, coroners have made scathing comments, and the medical journals have demanded legislative interference, but the sale of this highly corrosive drug continues merrily. Last week, at a Battersea inquest, it was proved that the wife of a chemist sold sixpenny-worth of carbolic acid to a man who said he knew it was a dangerous poison, and who afterwards applied that knowledge effectually in ending his own life. The deceased had said, as a matter of course, he wanted the stuff for disinfecting purposes. Why is this terrible drug not placed on the list of scheduled poisons forthwith? Of course, it is clear that any determined person bent upon suicide will find some means or other of carrying out his unhappy purpose. For all that, if the principle of registering poisons be a correct one generally, then it must apply to carbolic acid in particular. Besides, many deaths are caused by misadventure, owing to the haphazard distribution of this largely useful but nevertheless deadly substance.—*Med. Press.*

Acute dyspepsia is most difficult to treat when it occurs in nuslings under the age of three months, before which time farinaceous food is badly borne, either leaving the intestine unchanged or increasing the dyspepsia. A gruel made of ground arrow-root, maize, or rice is, perhaps, less intolerable than other forms, but in many cases one is reduced to feeding the infant, for a day, at least, on thin veal-broth or egg-water (the white of one egg, with some milk-sugar, to two pints of boiled water). If this diet is properly assimilated it may be continued for a couple of days, the child then being put on to a mixture of a pint of cows' milk with two pints of five-per-cent. solution of milk-sugar. In some instances peptonized or albumose milk may be given with advantage.—*Pediatrics.*

Headache Dependent upon Ovarian Disease.

A prescription of Dr. Sinkler's is
R Ammon. bromid 3v.
Ext. hydrastis fl. 3ss.
Tr. gentian. co 3ss.
Aqua 3iv.
M. Sig.: Dessertspoonful three times a day.—*Medical News.*

MR. CARLESS operated on a woman, aet. 50, who had for two years noticed a tumor growing in the upper part of the chest. It caused no pain nor inconvenience except on account of its size and prominence, and was as large as a fist, growing from and involving the manubrium; it was clearly defined and limited, and there was no indication of pressure on the mediastinal or thoracic contents; there was no embarrassment to the breathing, no difficulty in swallowing, and no edema of face, neck or arms. The patient, a somewhat alcoholic subject, looked fairly strong, and had not lost flesh to any extent. There was slight emphysema of the lungs. The risks of interference were pointed out, but she was willing an attempt should be made to remove the mass. The patient having been anesthetized by chloroform, Carless made a curved incision along the upper border of the growth, about six inches in length; this was carried well down in the middle line, to enable the finger to be inserted behind the tumor, and thus give some idea as to the feasibility of completing the removal. Apparently the mediastinal tissues, though adherent to the mass, were not extensively encroached on, and, therefore, the operation proceeded. A vertical incision was made in the middle, meeting the first incision, and the flaps thus marked out were dissected back. The insertions of the sterno-mastoids were divided on either side, and the clavicles sawn across close to the sternal articulation; the intercostal muscles in the first and second space were divided, and the first and second ribs having been separated from the structures lying behind them by a raspatory, were cut across with pliers. The next step consisted in clearing the back of the sternum on a level with the second intercostal space, and this was accomplished without difficulty by means of a long probe. The junction of the manubrium and gladiolus was divided by a saw, the tissues lying behind being kept out of the way by means of the probe. The tumor was now free except posteriorly. It was without difficulty lifted from its bed to a sufficient extent to enable the surgeon to see the posterior connections. Mr. Carless dealt first with those on the right, dividing the triangularis sterni, securing the internal mammary vessels, and separating the mass from the right pleura. Unfortunately, this was so intimately adherent to the growth that it was torn in several places during the proceeding. The right lung did not collapse entirely, yet it was seen moving vigorously up and down, and the respirations were more or less interfered with. The patient's pulse was beginning to flag. It was also found that the growth was adherent to a smaller extent on the left side, and hence Mr. Carless decided to desist with further interference, as the risk involved by having both pleural cavities open would be too great. The tumor was left in situ, and steadied by wiring the second ribs in position. The patient's condition was now so bad that it was thought unwise to attempt

to wire the clavicles in position. The wound was dressed and the arms fixed carefully to the sides. Mr. Carless considered that he had been fully justified in attempting removal of the growth. Keen, of Philadelphia, had recently collected a number of cases in which tumor of the sternum had been safely dealt with, and the absence of all pressure symptoms in this case suggested that it might be feasible to deal with the growth in spite of its great size. The failure, he said, was due to the extensive nature of the posterior adhesions, and the lesson that the case had brought him was that tumors of the sternum may be dealt with so long as they are unilateral and presumably only adherent to one pleural sac; if, however, they are central and sufficiently large to probably involve both pleura, no attempt at removal should be undertaken.—*Med. Press.*

It is of considerable importance that food or drink should be of the right temperature. For healthy people, hot food should be served at a temperature about that of the blood, for infants it is imperative that milk should be given at blood heat. Drink intended to quench thirst should be at a temperature of from 50° to 70° F. Drink or food at extremely high or extremely low temperatures may do great damage, and are most harmful when swallowed rapidly. Drinking water is best taken at 55 degrees, seltzers and soda water should be slightly warmer, and beer should not be cooled to more than 60 degrees; red wine is best at 65 degrees; white wine at 50; champagne is the one liquor which is best at the lowest temperature allowed, but should not be taken colder than 45 degrees. Coffee and tea should not be taken hotter than from 105 to 120 degrees; milk is considered cold at 60 degrees, when it will be found to have the best aroma.—*Exchange.*

E. R. Kirby suggests in treatment of congenital malformation of rectum:

1. An operation should always be performed and without delay.
2. If there be any chance of establishing an opening at the normal site of the anus, the surgeon should first direct his attention to this procedure.
3. The use of a trocha as an aid in finding the rectal pouch before or after incision through the perineum is not sanctioned by modern surgical authority.
4. The result of attempts to establish an outlet for an imperforate rectum through the perineum are not favorable as regards the production of a useful anus.
5. In case of failure to establish a new anus in the anal region, colotomy should at once be performed.
6. In the formation of an artificial anus the left groin is the best site for the operation.
7. Attempts at establishing an anus in the anal region after a colotomy are attended with great danger, and are generally unsuccessful.

J. B. Learned at the recent meeting of the British Medical Association, gave his experience with the many methods of **inviting sleep without taking drugs**. He detailed the positions of the body after retiring which he employed. He said the cause of delay in sleep coming is generally the brain running automatically without our consent, after we go to bed. He sets the brain to work at once on retiring—it is to direct the respiratory process. It is to count respirations to see that they are fewer in number, regular, deep and somewhat protracted. In addition, certain groups of muscles are employed in routine order in silent contraction. By constant change other groups are brought into use. He has completed a systematized routine of contraction and relaxation. A slight elevation of the head from the pillow for definite time by count of respirations is one of the many changes of position. All this is without any commotion, and need not be recognized by a sleeping companion. Brain and muscle and all parts of the body soon come into the normal state that precedes and invites sleep. A sense of fatigue soon overtakes one while thus employed, and before he is aware, the brain has forgotten its duty to regulate the breathing process, the muscles have ceased to expand to the call made upon them in the beginning, and sleep is in control of all the organs. The details of this method of inviting sleep will not be the same for the strong and the weak. The principle, however, is one and the same, viz., the proper direction of vital energy to brain and muscle, according to the condition of the individual. The effect of the brain and muscles combined, under direction of will, counteracts the one-sided automatic whirl of a little portion of gray matter that has come to antagonize normal sleep and to make night a source of gloom and unrest.—*Sci. Amer.*

An interesting discussion took place at the German Surgical Congress on the present position in respect of the surgical treatment of the **hypertrophied prostate**. It is conceded that in four-fifths of the cases treated by castration the gland was reduced in size, and this proportion roughly corresponds with the results obtained by Brun and Socin from resection of the vasa deferentia. Generally speaking, the effects of the operation were manifested with great promptitude, but in a few cases several months elapsed before the symptoms began to subside; while in a small minority no tangible improvement was obtained. According to Guyon, prostatic hypertrophy is not a disease *per se*, but only one manifestation of a morbid process associated with arterio-sclerosis, which involves the whole genito-urinary apparatus. Whether this hypothesis be correct or not, it is agreed that treatment must not be limited to the prostate. According to Socin, in eighty per cent. of the cases of hypertrophied prostate discovered post-mortem, the

patients had never presented symptoms of urinary troubles, from which fact he infers that the enlargement of the prostate alone does not constitute **prostatism**, a condition in which an abnormal condition of the bladder plays an important part. He goes so far as to assert that the diminution in size of the prostate which follows one or other of these operations is not due to atrophy, but merely to decongestion, a result which he asserts can be equally well obtained by massage of the organ. It may be conceded that in a certain but variable proportion of cases of prostatic hypertrophy some measure of relief is afforded by castration or resection of the vasa deferentia; but in young patients, that is to say men under sixty years of age, these operations are apt to determine grave mental disturbance of a melancholic type, and in Germany at any rate, the direct mortality of the operation is tolerably high.—*Med. Press.*

BEYER (*Fortschrit. de Medicine*, No. 1, 1897) has tested different methods employed for the **disinfection of bed linen and underclothing**. The ordinary methods by boiling are not suited to these articles, as the presence of blood, pus, and feces causes an ineradicable stain if a high temperature is used. Soaking the garments in solutions of various soaps for one or two days failed in every instance to kill cholera, typhoid, and pyogenic organisms which were mixed with the feces with which the garments were smeared. In some cases the germs were killed when the solutions containing the linen were kept at 50° C. for a few hours. With lime-water the results were much better. Sample garments which were soaked in this solution for twenty-four hours were found to be sterilized. An equally good result was obtained in a hospital where about one-half a cubic meter of soiled linen was soaked in lime-water for soaked in this solution for twenty-four hours if the clothing was first rinsed with lime-water and then placed in a fresh solution. The lime-water does not injure linen or cotton goods, but shrinks woolen to such an extent as to prevent its use.—*Med. News.*

Surgeon Parke, who accompanied Stanley's Emin Pasha relief expedition, declares that the men of the company who had been vaccinated before setting out escaped an epidemic of small-pox which broke out in the wilds of Africa, while the unvaccinated camp followers took a malignant form of the disease, and many of them died.

W. S. Gottheil (*Int. Jour. Surgery*) condemns the **use of caustics in any warty growth** of the genital organs. Their use may induce a malignant growth in the place of a benign. The knife or actual cauterity should be the means of treatment of these growths. Total extirpation or let alone.

NEWS AND MISCELLANY.

At the Fourth Annual Meeting of the American Medical Publishers' Association the following resolution was adopted:

Whereas, The Imperial Granum Company has announced the withdrawal of all its advertising from the lay press, and signified its intention of using medical mediums only in the future, therefore be it

Resolved, That the American Medical Publishers' Association, in session at Philadelphia, hereby endorses and commends this action of the Imperial Granum Company, and further recommends this course to other manufacturers who desire the support and co-operation of the medical profession.

George Beatson, of Glasgow, writing on the relations between the organs of generation and cancer, recounts a case of **mammary cancer cured by removal of the ovaries**. In explanation of the reasoning which induced him to perform the operation, which he did with success, he mentions that his attention had been attracted by the close relation existing between the mammae and ovaries in animals; the function of one or other being active means that the function of the other is in abeyance for the time being, and vice versa; also the close similarity in the production of milk by the metabolism of the glandular cells and the change occurring in these same cells in cancer formation; the theory being that the epithelium of the ovaries preserves an embryonic character and a germinating power which is not lost as it is in different cells of other structures, except the latter retrograde towards the embryonic state. If Beatson's view be the true explanation of cancer formation, those who look for bacterial origin must needs be disappointed.

Cancer is not inoculable, and no specific bacterium or organism has been cultivated from which cancer can be propagated. Failing in this, the microbe theory fails. Beatson's view is supported by the pathology of other germinal histoid growths, enchondroma of the testicle for example, for the origin of which we go back to the blastoderm. The form and mode of generation of cancer cells favor the assumption, though it is difficult to explain the autoinfectivity and fatal nature of the disease on these data. The inception of the idea and practice detailed is interesting and unique in the annals of pathology and surgery.—*Med. Press and Circ.*

Renal Colic.

Wittzack orders

B Lyeetol gr. xv.
Sodii bicarb. gr. viii.
M. ft. chart. no. j.

Sig.: To be taken morning and evening, dissolved in a glass of mineral water.—*Medical News.*

Health Reports.—Received in the office of the Supervising Surgeon-General of the United States Marine Hospital Service during the week ended October 30, 1897:

YELLOW FEVER—UNITED STATES.

ALABAMA.

Mobile, Oct. 23-29, 40 cases—5 deaths.
Montgomery, Oct. 21-29, 99 cases—6 deaths.
Notasulga, Oct. 25, 1 case.
Sandy Ridge, Oct. 25, 1 case.
Selma, Oct. 23-26, 17 cases—1 death.
Whistler, Oct. 29, not stated—2 deaths.

GEORGIA.

Atlanta, Oct. 24, 1 case—(refugee).
Oct. 27, 1 case—(detention camp).

LOUISIANA.

Baton Rouge, Oct. 22-28, 3 cases.
Franklin, Oct. 20-21, 2 cases.
New Orleans, Oct. 23-29, 367 cases—52 dhs.
Petterson, Oct. 21, 1 case.

MISSISSIPPI.

Bay St. Louis, Oct. 23-28, 33 cases—3 dths.
Biloxi, Oct. 23-29, 91 cases—5 deaths.
Cayuga, Oct. 23-28, 5 cases.
Clinton, Oct. 23-29, 7 cases.
County Farm, Oct. 25, 1 case.
Edwards, Oct. 23-29, 19 cases—1 death.
Hinds Co. convict camp, Oct. 23-27, 6 cases.
McHenry, Oct. 23-29, 6 cases.
Nitta Yuma, Oct. 23-24, 6 cases.
Pascagoula, Oct. 23-29, 11 cases.
Scranton, Oct. 23-29, 75 cases—4 deaths.

TENNESSEE.

Memphis, Oct. 23-29, 31 cases—3 deaths.

TEXAS.

Galveston, Oct. 27, yellow fever present.

YELLOW FEVER—FOREIGN.

BRAZIL.

Para, Oct. 2-9, 5 deaths.

CUBA.

Cienfuegos, Oct. 10-17, 2 deaths.
Habaria, Oct. 14-21, 13 deaths.
Matanzas, Oct. 6-20, 6 deaths.

JAMAICA.

Kingston, July 9-Oct. 9, 44 cases—23 dths.
Manchester, July 9-Oct. 9, 7 cases—2 dths.
Port Antonio, July 9-Oct. 9, 1 case—1 dth.
St. Elizabeth, July 9-Oct. 9, 1 case—1 dth.

SAN SALVADOR.

July 1-31, 38 cases—11 deaths.
Aug. 1-31, 54 cases—16 deaths.
Sept. 1-30, 34 cases—12 deaths.

Expressed in time units, the distance between Cape May, N. J., and Philadelphia, is 100 Minutes—measured by the "Century Flyer" over the route of the South Jersey Railroad.

This, and like marked reductions in time to other points, in connection with the superior modern equipment, splendid service, and capable management maintained by the railroad, easily accounts for recent great increase of travel to the health resorts along the southern coast of New Jersey.